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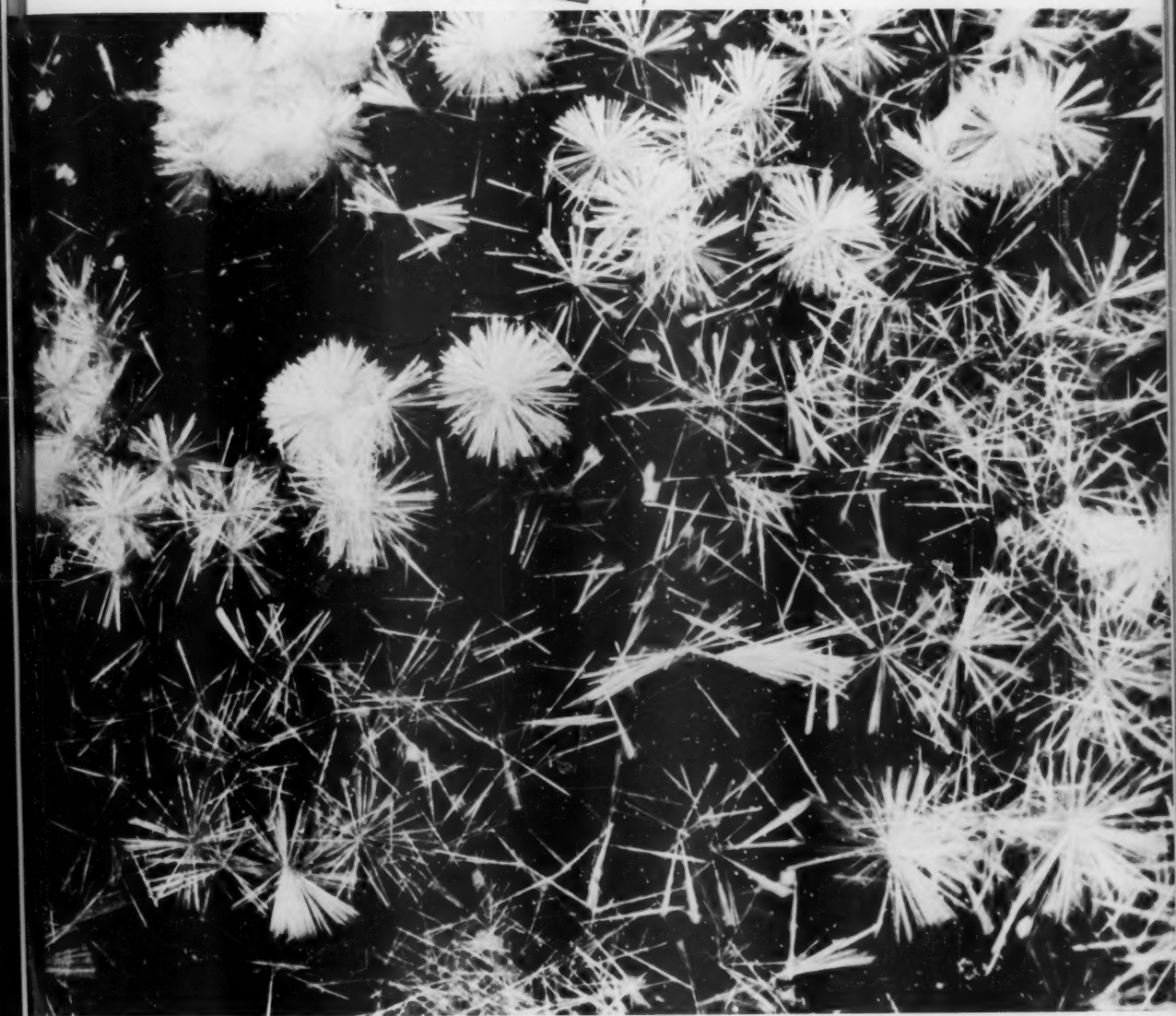

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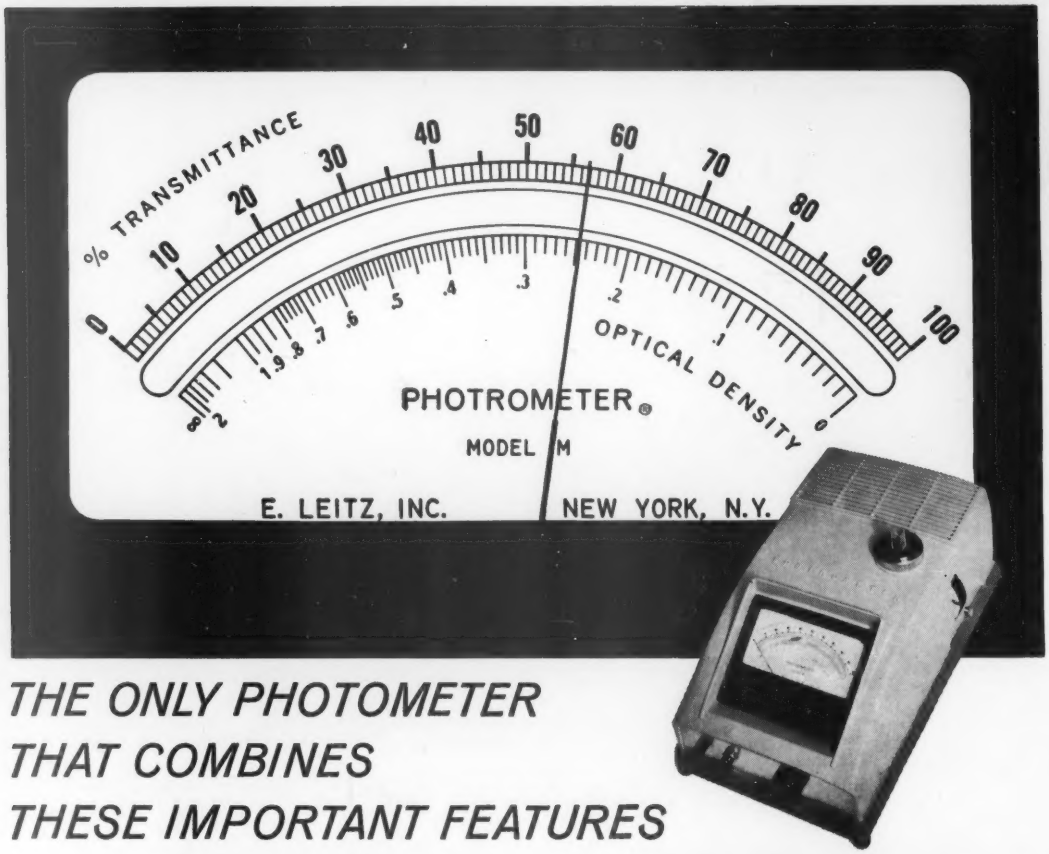

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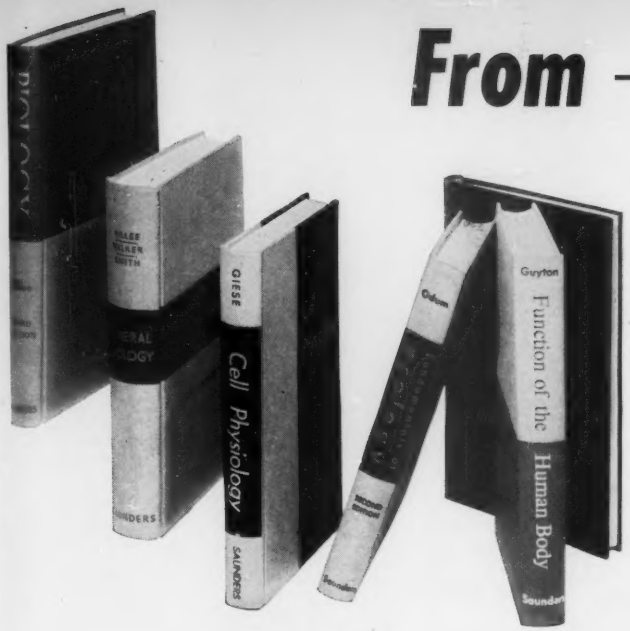

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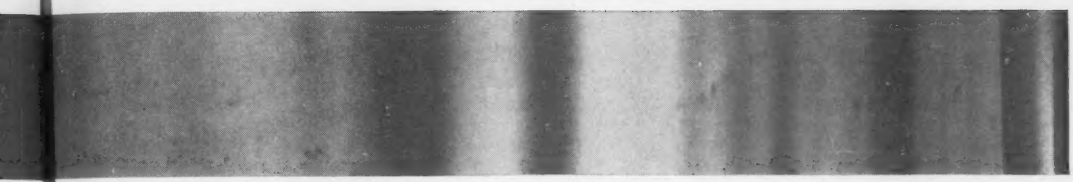

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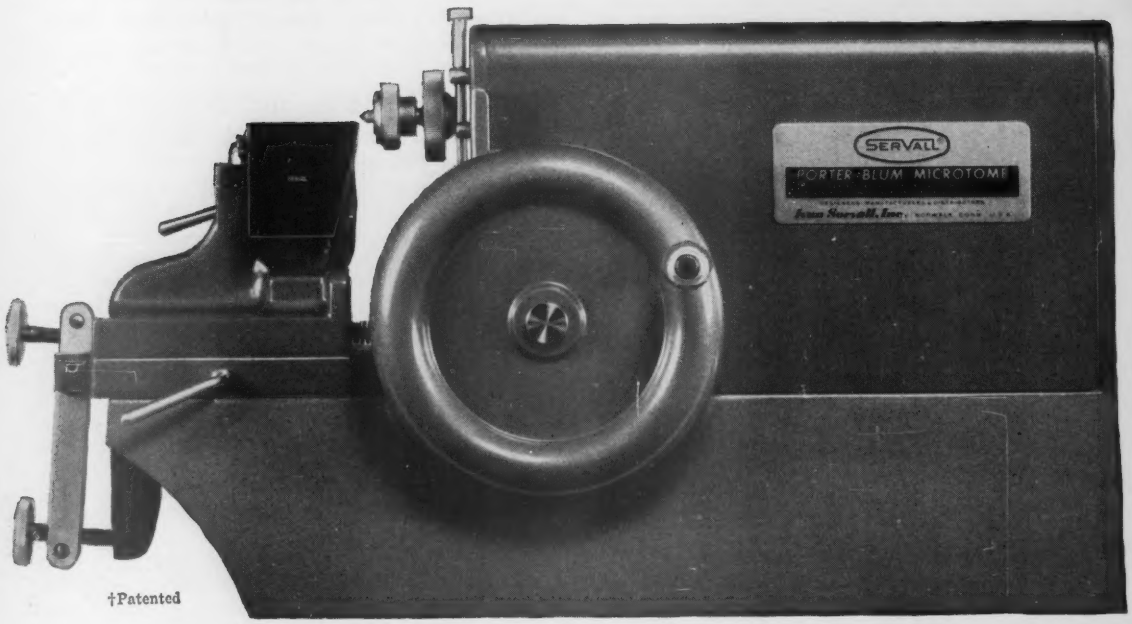

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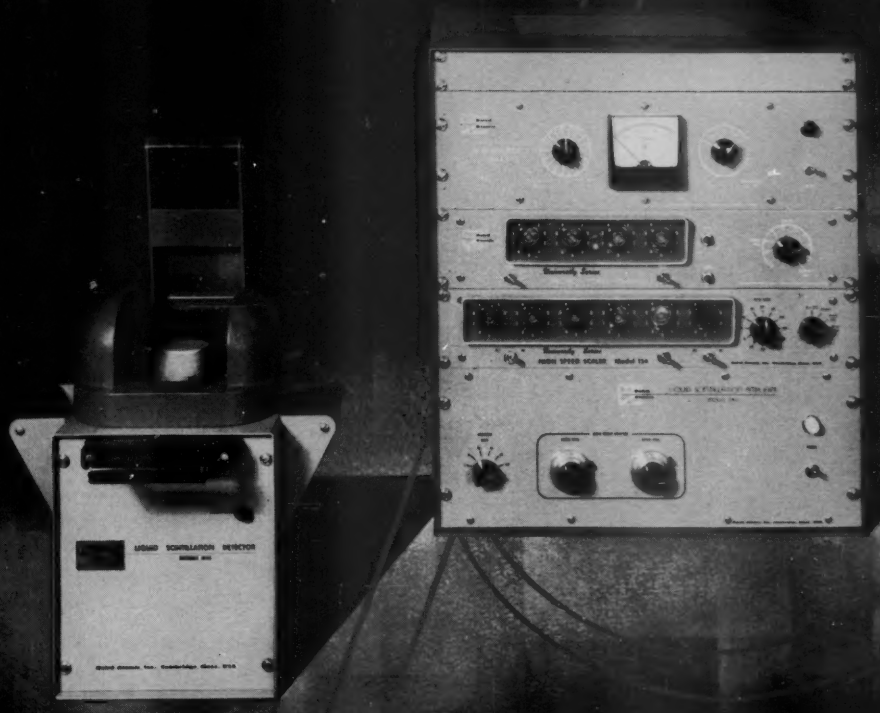

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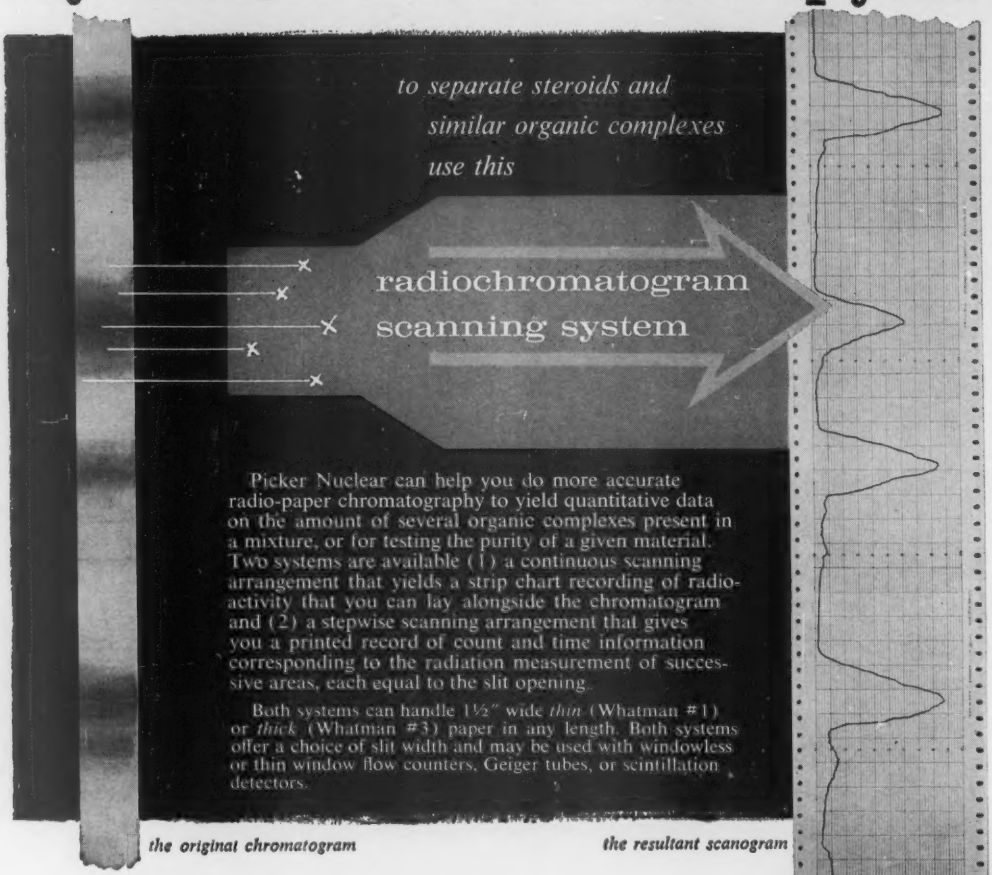
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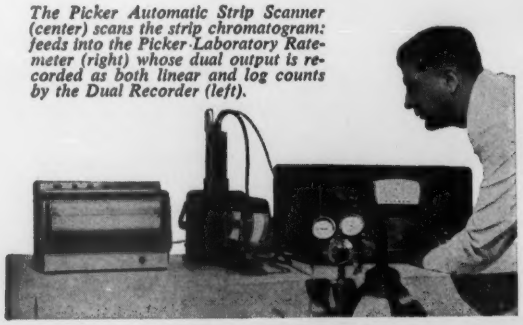

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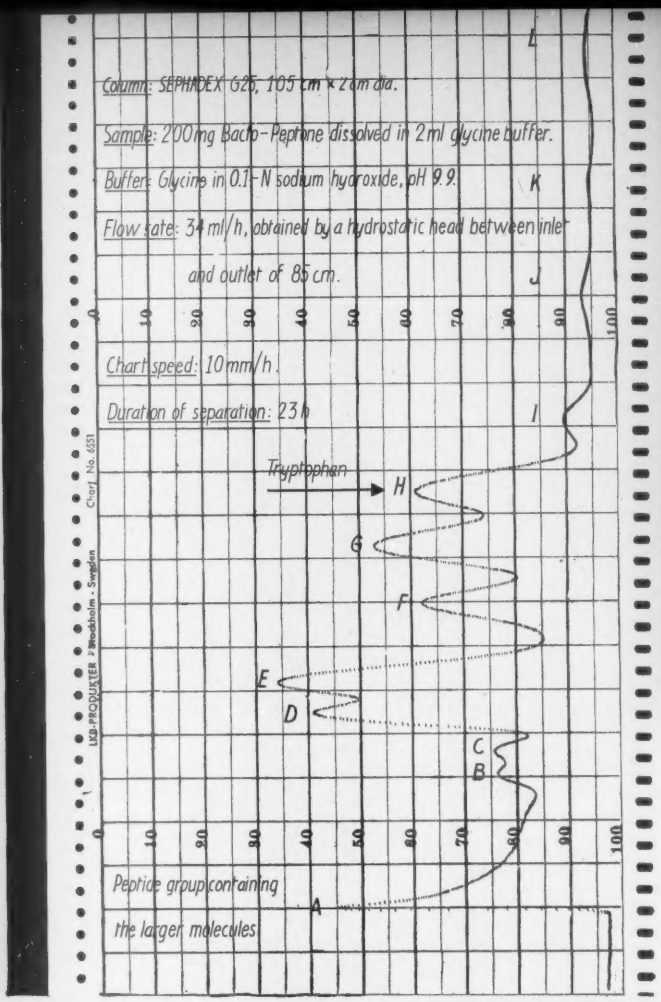

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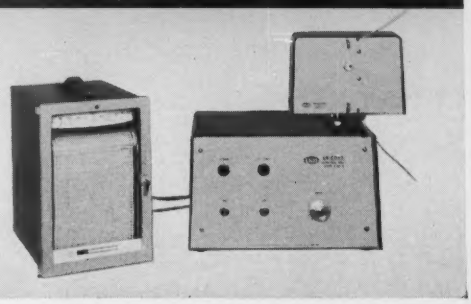

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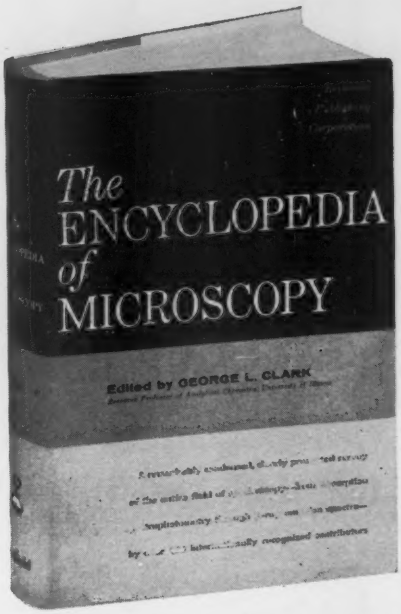

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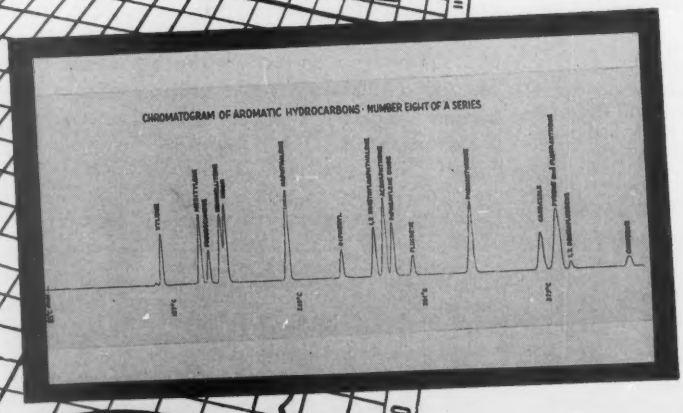

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Until recently, the application of gas chromatography to aromatic compounds derived from coal tar was restricted to mixtures boiling below 300°C[1,3]. Due to the complex and wide-boiling nature of these mixtures, several chromatograms had to be run at different constant column temperatures to separate all of the components. Much of the gas chromatographic detail for the higher boiling components is lost due to peak spreading and it is difficult to use the data either qualitatively or quantitatively.

The use of linear programmed temperature gas chromatography, however, enables analysis of such complex mixtures in a single run and also facilitates both qualitative and quantitative analysis. The chromatogram at the left illustrates this for the analysis of a 17-component mixture of aromatic hydrocarbons ranging from xylene (b.p. 139°C) to chrysene (b.p. 448°C). The accuracy of the analysis is shown in the following table.

QUANTITATIVE ANALYSIS

Compound	Added, %		Found, %
Xylene	5.9		5.5
Mesitylene	5.9		6.4
Pseudocumene	2.9		2.9
Hemimellitene	8.8	14.7	14.0
Indan	5.9		
Napthalene	8.8		9.7
Diphenyl	2.9		3.5
1,2-Demethylnaphthalene	5.9		5.9
Acenaphthene	8.8		9.6
Diphenylene oxide	5.7		6.0
Fluorene	2.9		3.3
Phenanthrene	8.8	13.2	11.9
Carbazole	4.4		
Fluoranthene	5.9		6.5
Pyrene	8.8		9.7
1,2-Benzafluorene	1.4		1.1
Chrysene	5.9		4.9

The last column lists the amounts of each compound determined by simple peak area summation. The excellent results are attributed largely to the good separation and the sharp peaks obtained by temperature programming a well-conditioned Apiezon L column.

This analysis was made on a Model 500 Linear Programmed High Temperature Gas Chromatograph[2]. This unit uses stable, hot-wire thermal conductivity detection for operation to 500°C. Separate and independent temperature controls for the column, detector, and injection port are also featured.

LITERATURE CITED

(1.) Dupire, F., *Z. Anal. Chem.*, **170**, 317-326 (1959).

(2.) *Martin, A. J., Bennett, C. E., and Martinez, F. W. Jr., "Linear Programmed Temperature Gas Chromatography," presented at the Cleveland ACS Meeting, April 1960.

(3.) Ta-Chuang Lo Chang, and Karr, C., *Anal. Chim. Acta*, **21**, 474-490 (1959).

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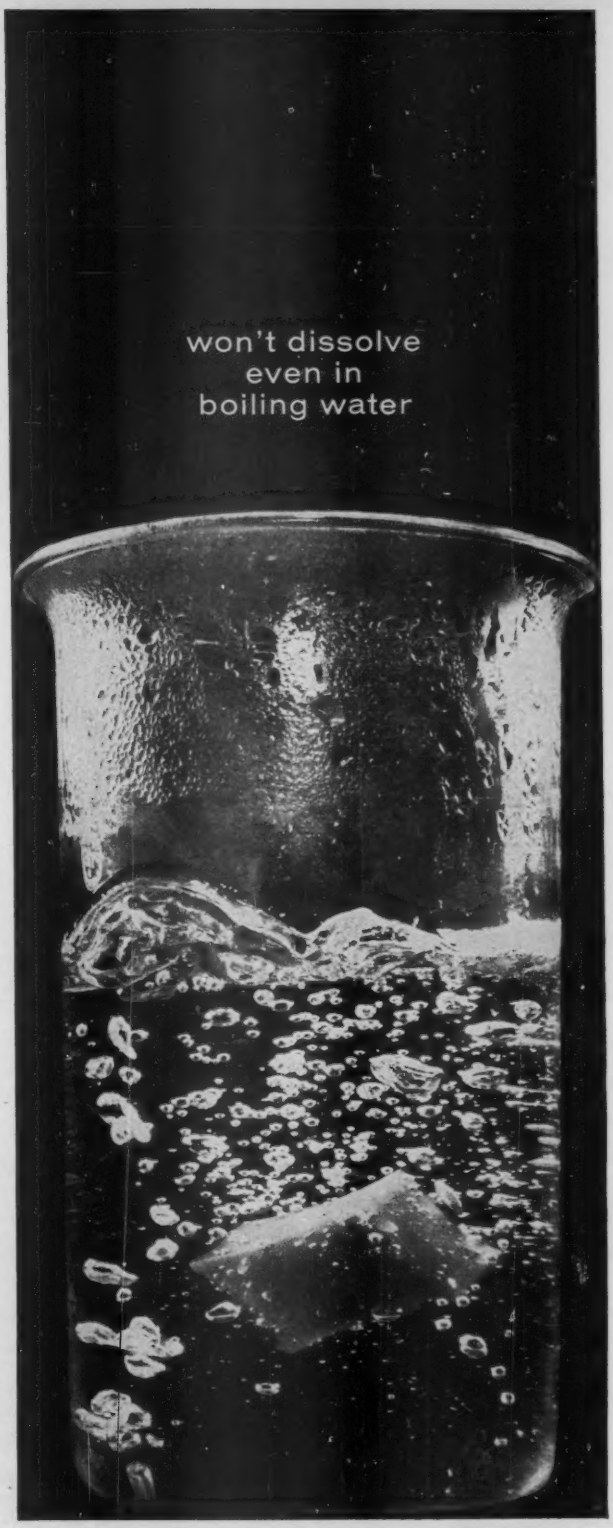

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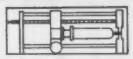
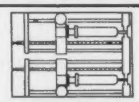
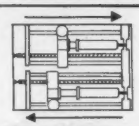
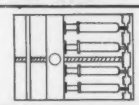
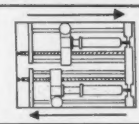
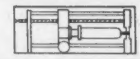
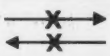
Model No.	Driving Mechanisms	Pumping Mechanisms	Solenoid Valves
600-900			
600-910			
600-920			
600-930			
600-950			
600-960			

BASIC PUMP MECHANISM

This unit consists of a movable carriage driven by a half-nut resting on a precision-cut lead screw. As the carriage can be lifted from the lead screw and moved to any position, loading can be accomplished in seconds. A small ramp can be locked to the side rail in any position. The ramp will raise the carriage from the lead screw, thus stopping the pumping action at a pre-set point. The adjustable syringe clamp will hold standard B-D Luer Lok syringes from 2 cc. to 50 cc. size. In order to achieve greater versatility, many pumps utilize more than one basic pumping mechanism.

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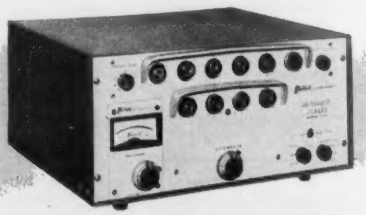

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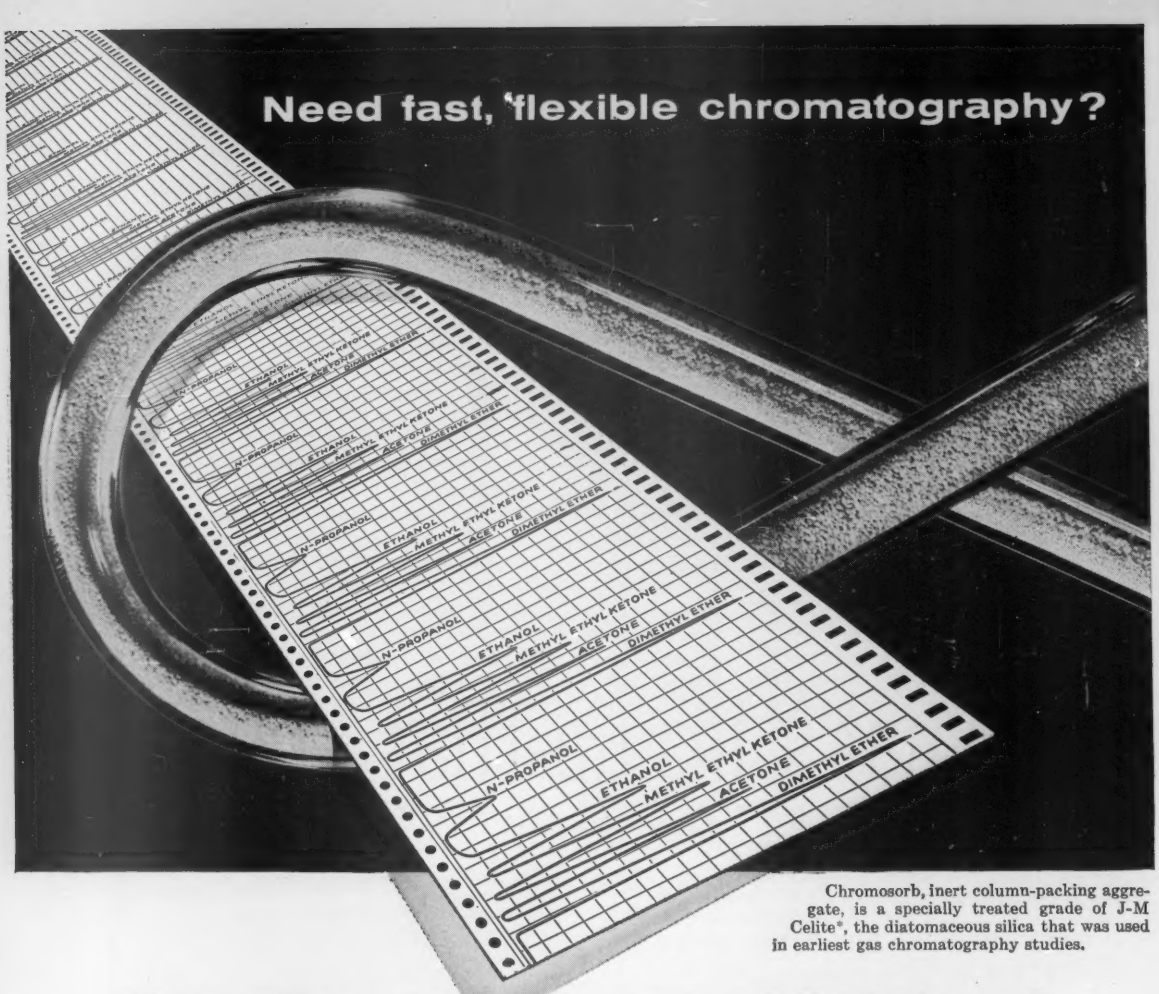

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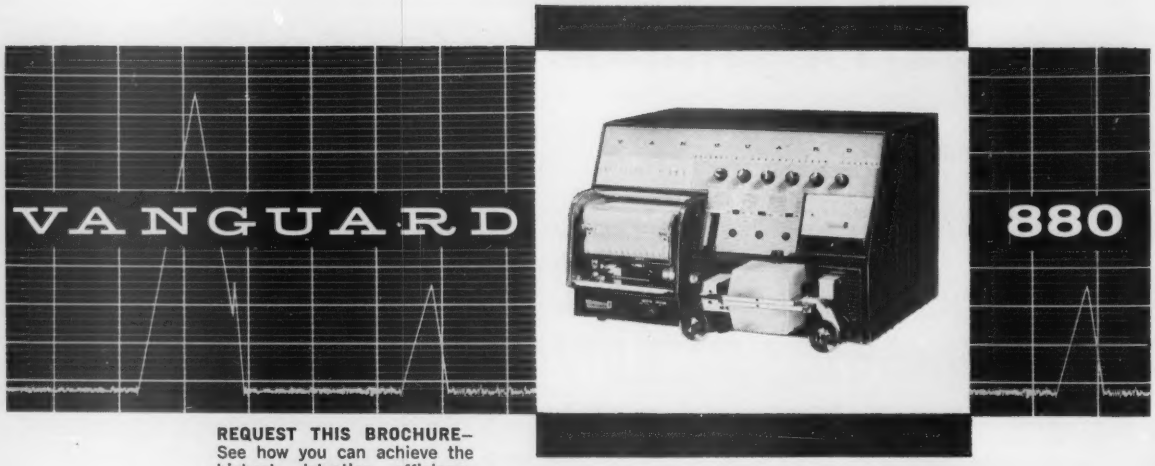

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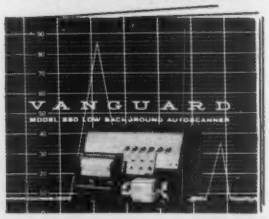

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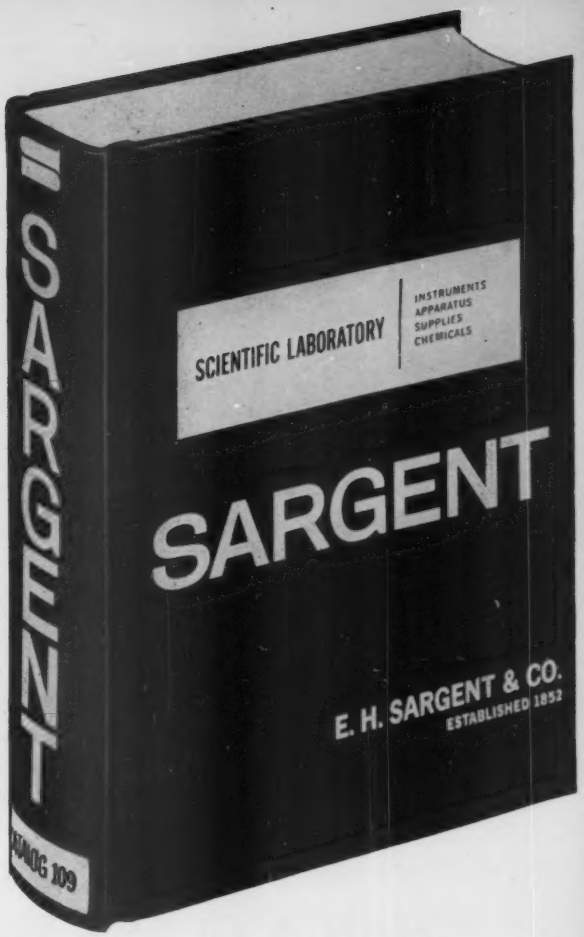

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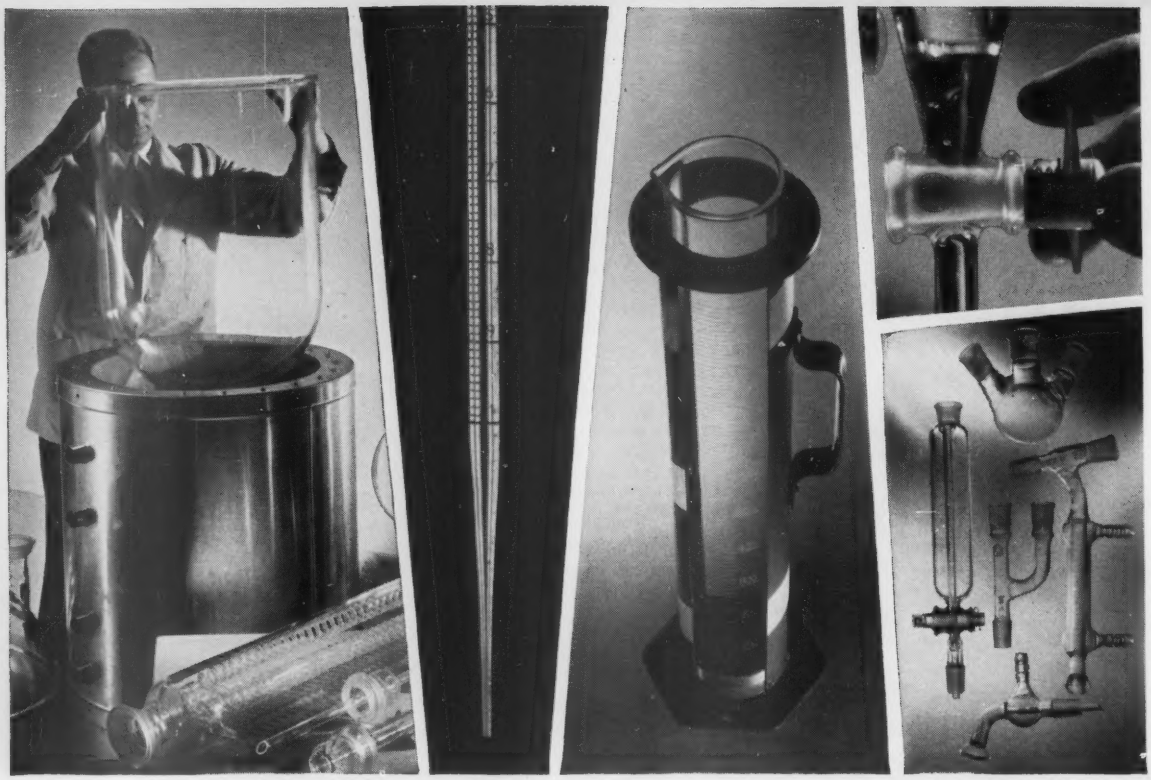

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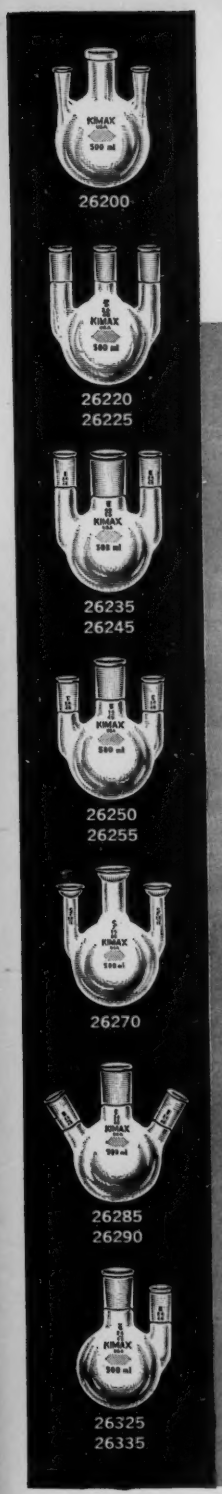

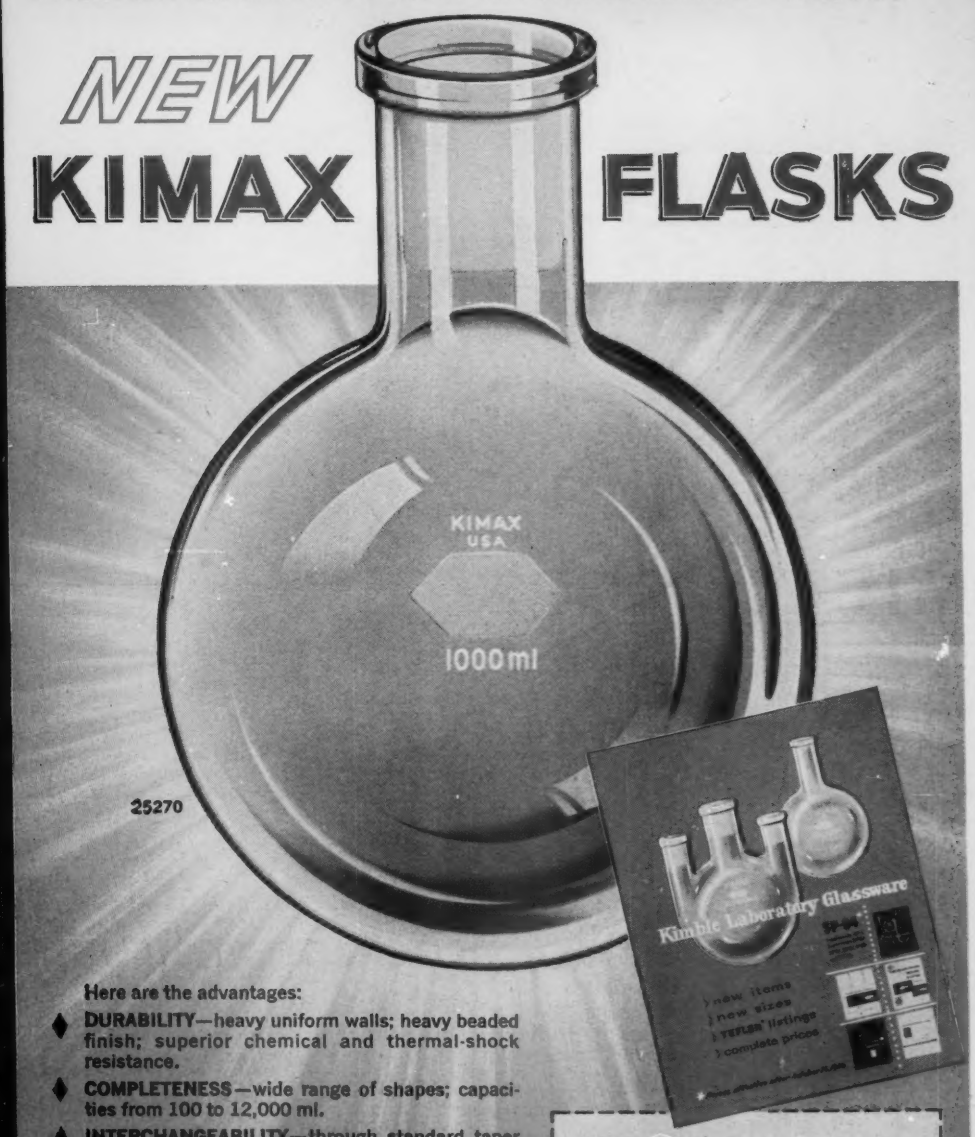

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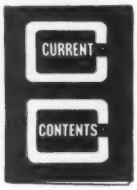

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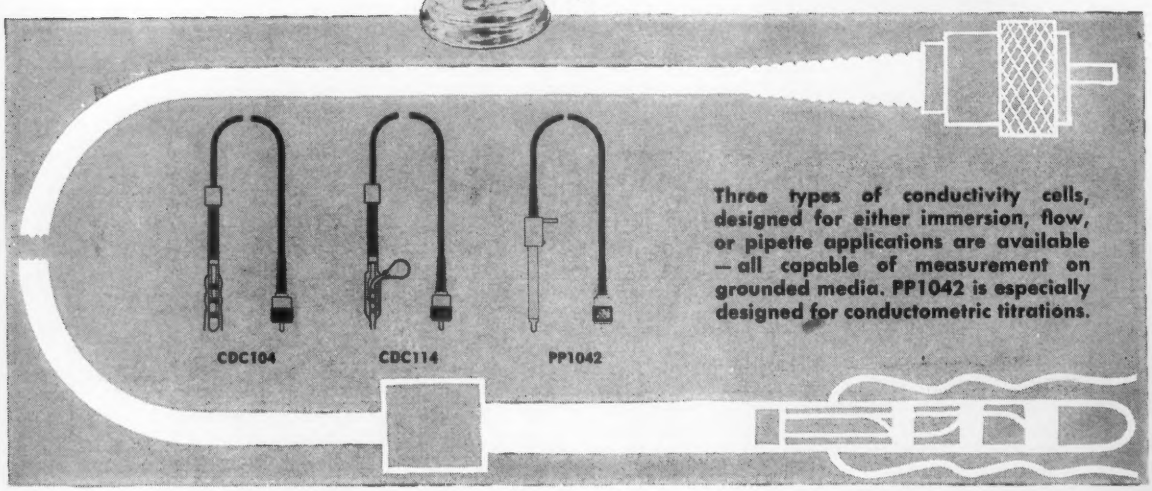

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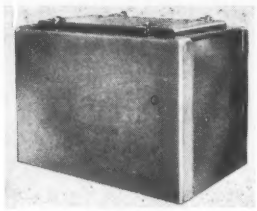

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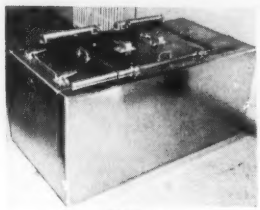

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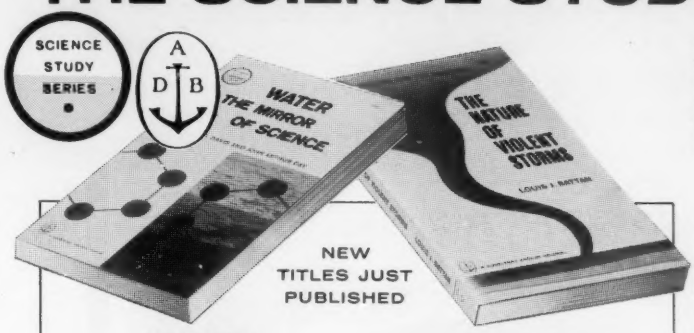

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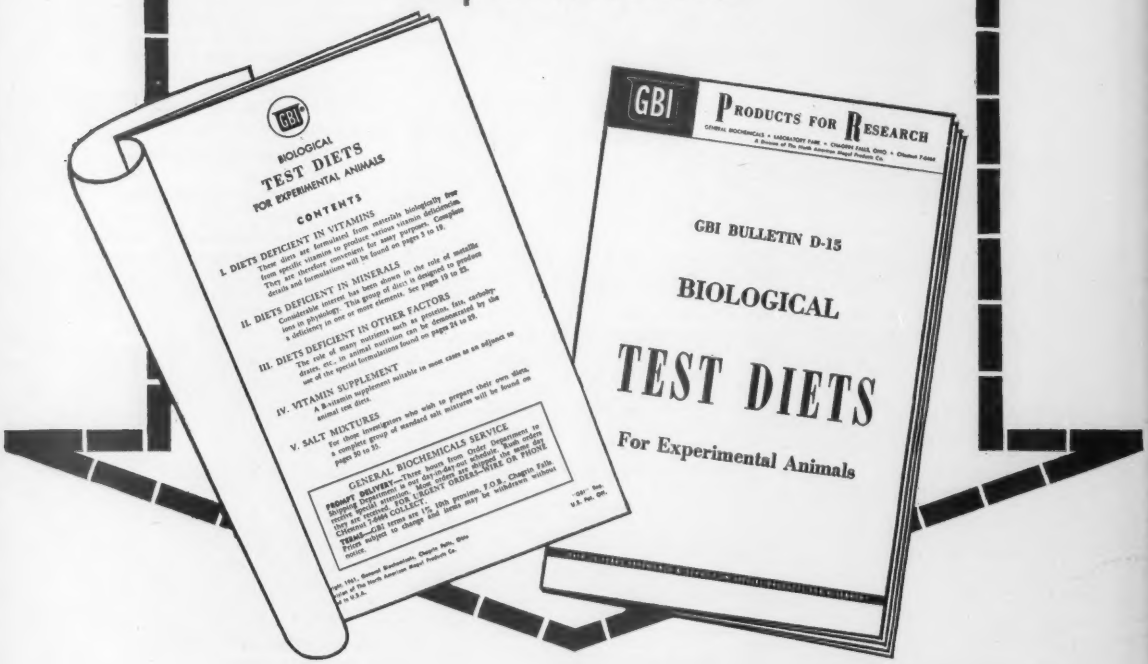

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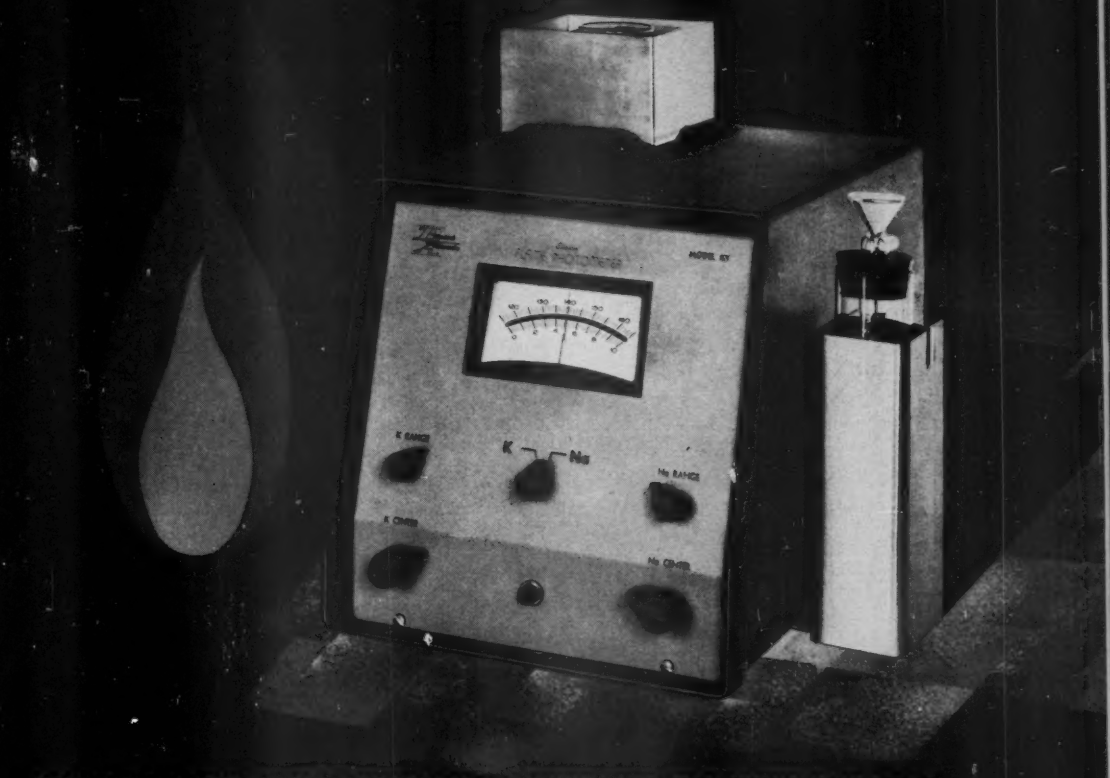

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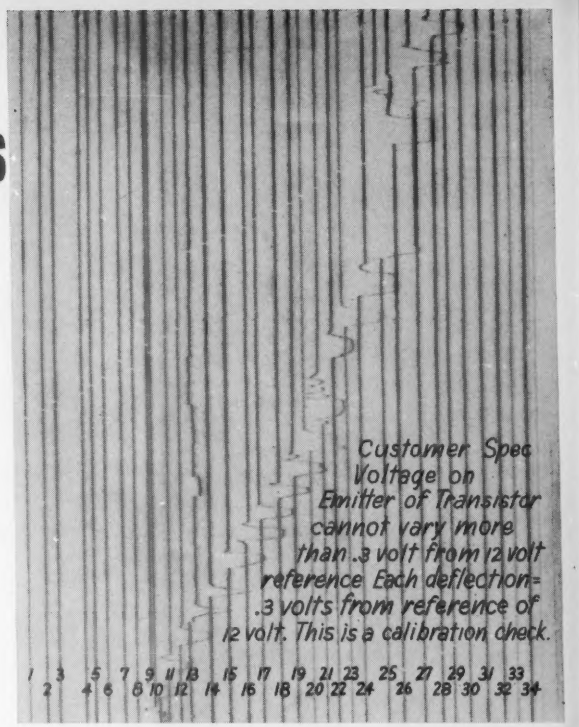
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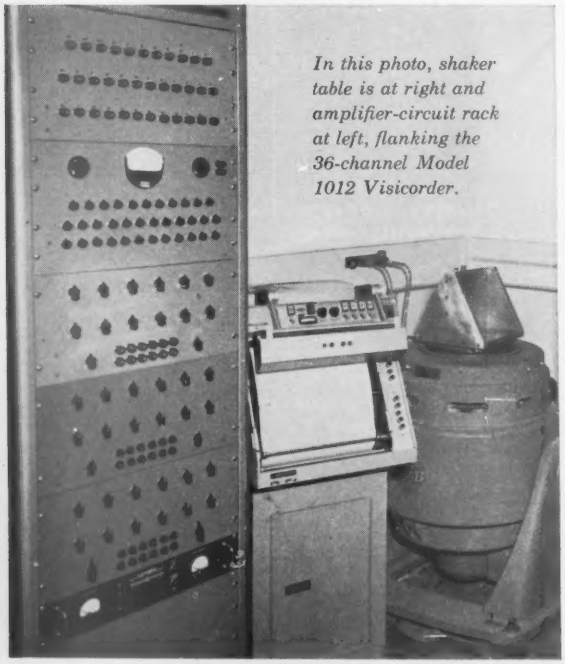

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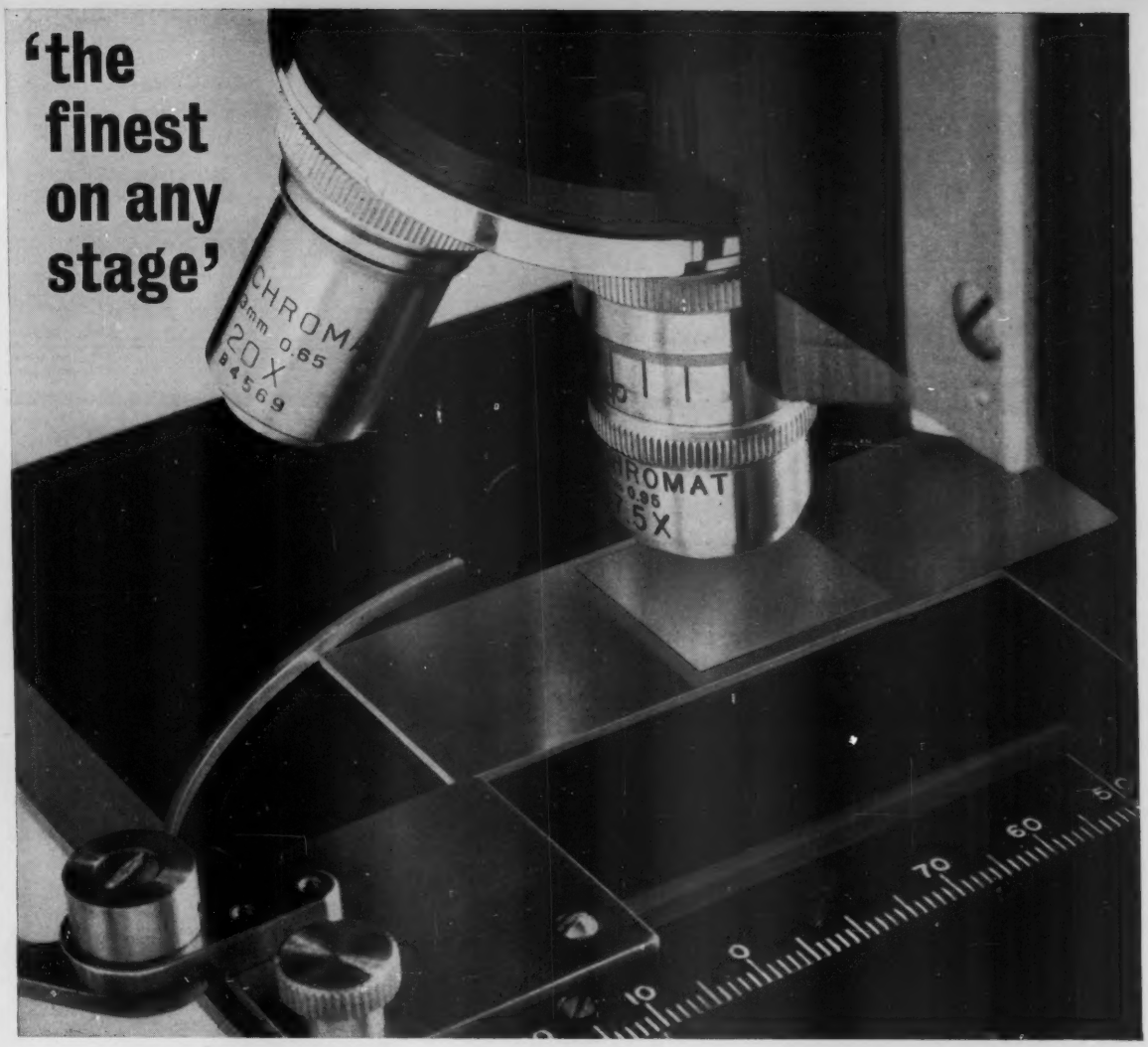

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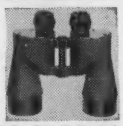

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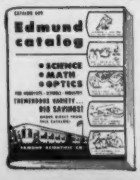

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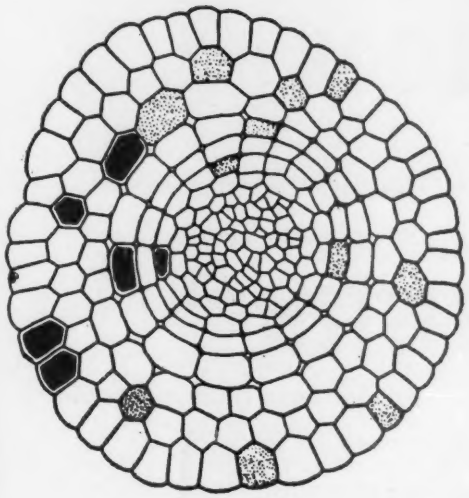

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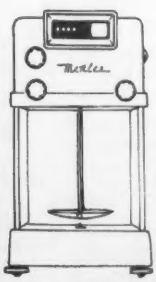

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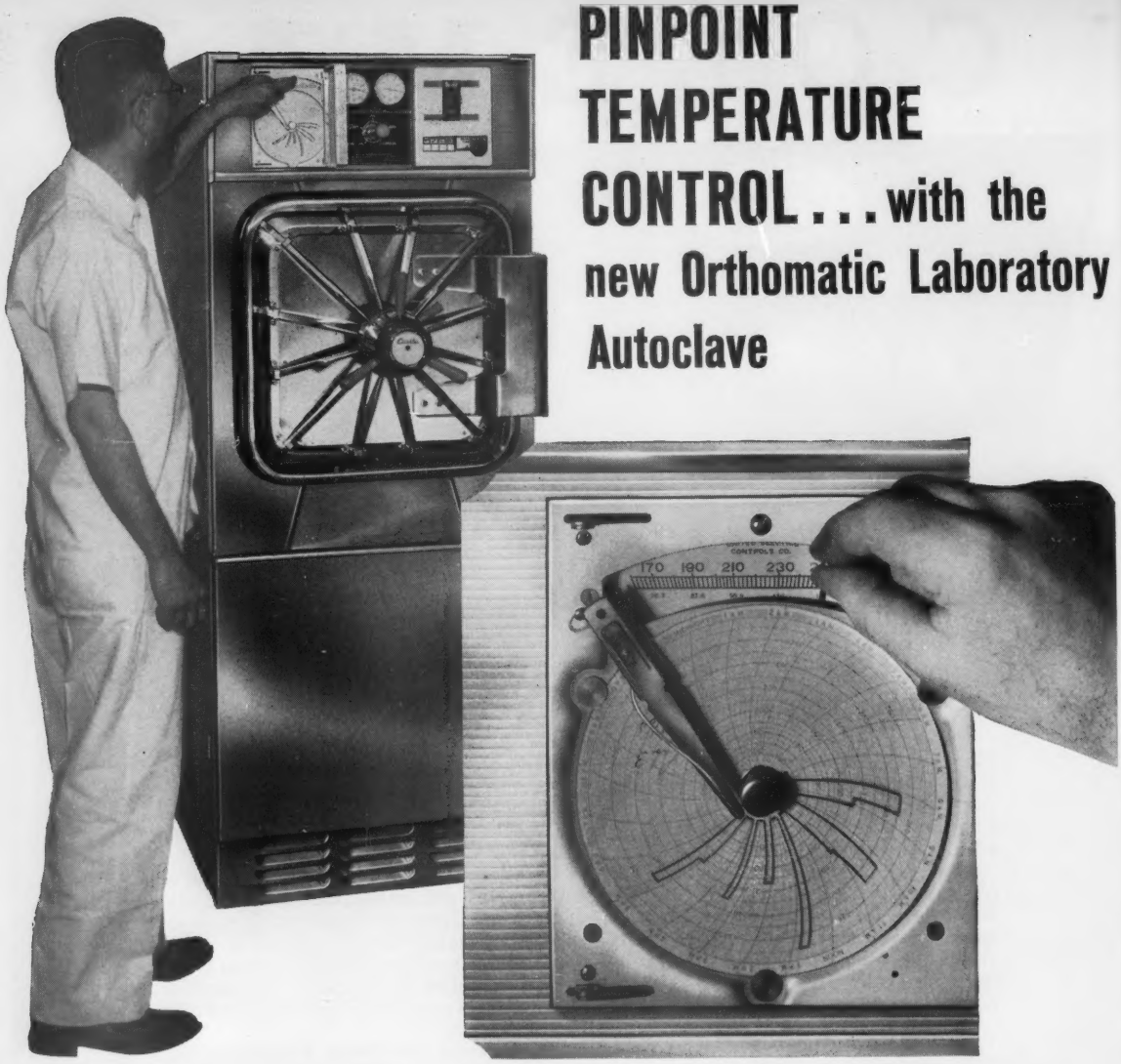

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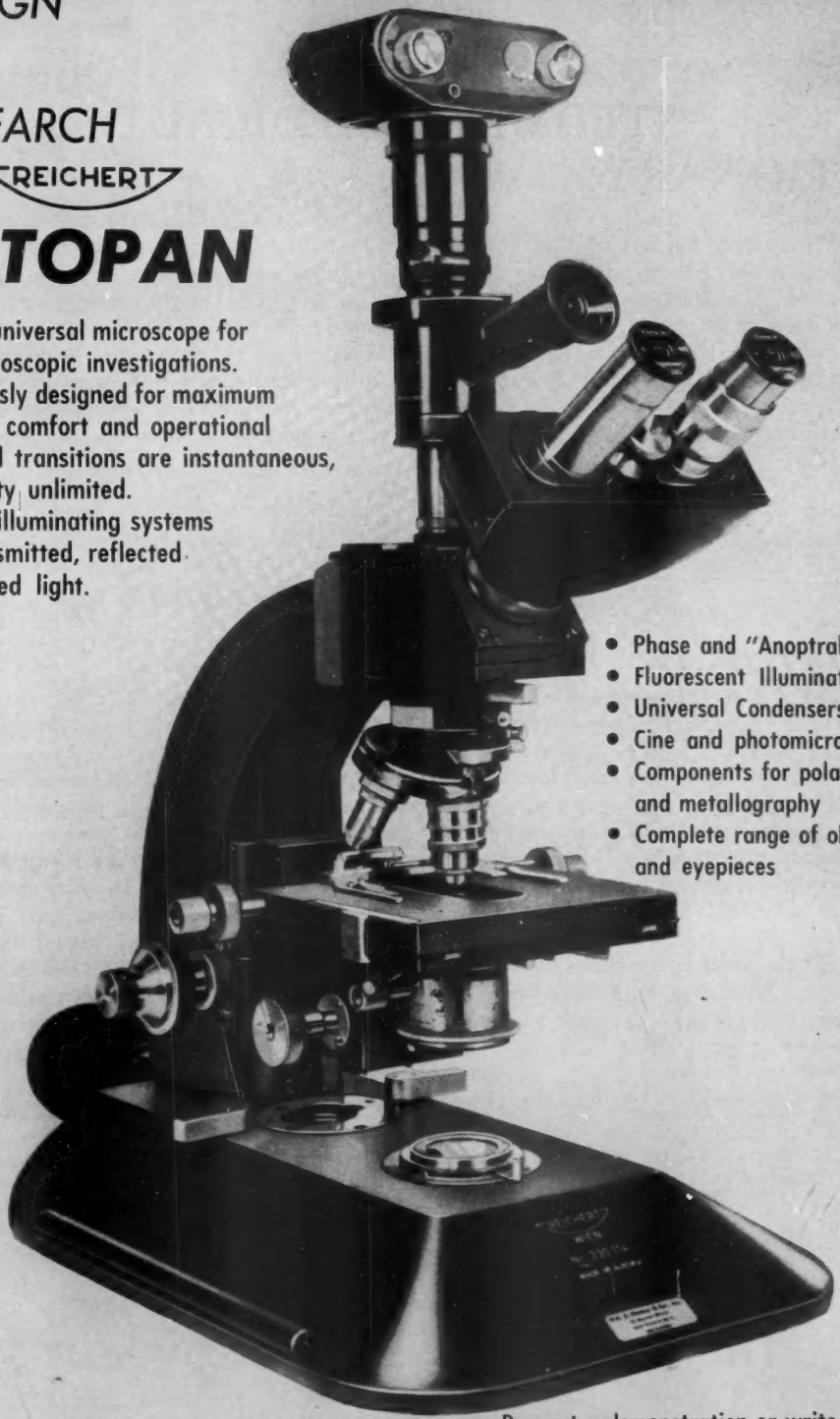

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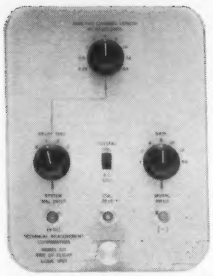

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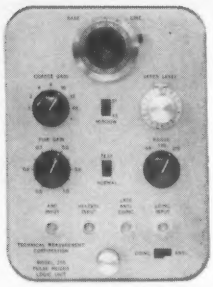

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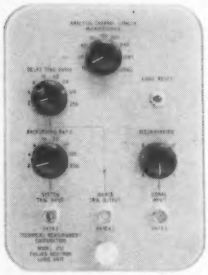

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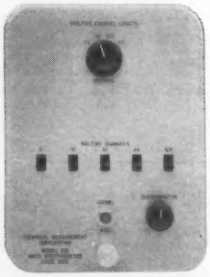

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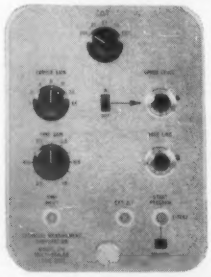

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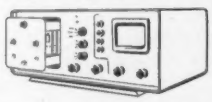

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What Laymen Can Ask of Scientists

In one of the essays in his last volume, *Resistance, Rebellion, and Death*, Albert Camus considers the question of what unbelievers like himself can ask of Christians, and his answer is relevant to the question of what laymen can expect of scientists. Camus warns first against a certain kind of hypocrisy, the hypocrisy of the man who does not accept the Christian truth but who, nevertheless, expects more of Christians than he does of himself. Christians have many obligations, according to the essay, but it is not the province of the unbeliever to recall them for the believer. Camus concludes, however, that he may permit himself to demand certain responsibilities of Christians—he has in mind, among other points, the failure of Rome to speak out immediately and clearly against political executions in Spain—but these responsibilities can only be those that must be required today of everyone.

The relevance of Camus's question to the question of what laymen can ask of scientists begins with the presence of similar opportunities for hypocrisy. Here also persons who do not follow a certain pursuit have the chance to indulge themselves by expecting special things of persons who do. Laymen can persuade one another that it is a simple matter to be a scientist and, from the vantage point of no personal involvement, expect scientists to be more selfless and lofty of purpose than they themselves are. Scientists and former scientists, it is true, sometimes encourage such expectations by saying that such virtues play an essential role in the scientific enterprise. There is the claim, for example, that there is something moral in the very grain of science. Scientists rarely fake experiments, and the reason, so the argument runs, is not that they are afraid of being caught in a lie by other scientists who try to repeat their experiments, but that scientists cannot tell lies and still achieve their objective of getting answers to the questions they put to nature.

There is an opportunity, then, to regard scientists as just a bit more saintly than other men, and so to make special demands of them. At the same time, interestingly enough, there is also an opportunity to regard scientists as just slightly greater sinners than other men, and so to make the same demands again. Men who are not scientists expect special atonement by scientists for the role they have played in the creation of many of the ills that now beset us. They demand atonement especially for the creation of that Sword of Damocles that hangs over us all, the atom bomb and the missile to deliver it. And again scientists have encouraged such demands.

But let us not give one another opportunities to indulge in hypocrisy. On the one hand, scientists may practice truthfulness in their work as a welcome necessity, but this does not mean that they embrace truth with equal fervor when it comes to seeking funds and recognition. And, on the other hand, in the application of science to military technology, it was not scientists who invented the occupation to which their talents have been applied with such stunning effectiveness. Any group, it is granted, has the prerogative to hold itself accountable in special ways. To paraphrase Camus, if there is anyone who can make demands of the scientist, it is the scientist himself. But what can laymen ask of scientists? As in the answer Camus gives to his question, there are obligations laymen can require of scientists, but these can only be the obligations necessarily required of any human being today, scientist or not.—J. T.

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21 April 1961, Volume 133, Number 3460 SCIENCE

Francis Bacon as Educator

He inspired the creation of the Royal Society and foresaw the rise of modern science.

Loren C. Eiseley

There are two colossal intellectual figures which loom across the century of Queen Elizabeth. Of one, William Shakespeare, we know almost nothing except that by some terrible insight into men, he understood almost as much about the human psyche as we have since learned painfully by scientific means. Of the other man, Francis Bacon, we think we know almost too much. We frown from the safe distance of four centuries at the intrigues of a mean court and moralize upon his fall, yet we are sufficiently overawed by the meteoric genius of both men that we sometimes seek to combine their talents in the body of one individual. The talents we are trying to combine represent, oddly enough, the two great academic worlds of today—the scientific and the humanistic. They were less divorced in the time of which we speak; in fact, one world, the scientific, was at best incipient, and the other, at least in the case of the drama, had the stain of lower-class vulgarity upon it.

Now Francis Bacon represents an odd phenomenon in science. He is frequently denounced, but we cannot lay his ghost. Within the last few months I have heard him castigated by legal scholars as a scoundrel whose personal life made anything he might have to say on ethical matters suspect. Similarly, on occasion I have heard Bacon criticized by scientists as a man who discovered nothing and who presented the world with an inductive method which, furthermore, would make it impossible, if consistently followed, to discover anything. In addition, Bacon has been excoriated as the propounder of a false idea that there is no aristocracy of scientific great men and that scientific research can be performed by common men pursuing certain principles. This, it has been contended, was the crowning folly of Bacon's particular world view. Why then, we may ask, in the light of these multiple charges, does this man, whose generation, in his own words, wore out days few and evil, who made no single great act of discovery, whose personal life is unsavory to our age (*1*), stand out with Shakespeare as one of the two greatest minds of a generation marked by great deeds and powerful personalities?

Last and most curious of all, why does Francis Bacon, whose four hundredth anniversary we are celebrating, continue to be tried and retried before the court of his intellectual descendants in every century since his death. Why cannot we let this poor ghost rest? Why cannot we render a final verdict? Is it because he left his name ambiguously to the "next ages" and because each age since his death feels it is the one to pronounce a verdict? Or is it that we, in trying this man, are unconsciously seeking to pass judgment upon our century and upon ourselves? Is it we who are both judged and the givers of judgment? Are we projecting outward upon a vacant chair and a phantom defendant, in this age perhaps more than any other, our fears, our hatreds, our almost willful misunderstanding of the world for which he spoke?

It is my contention that this psychological projection is a fact, and I shall assume the case for the defendant. Let it be understood at the outset, however, that each of us is both the judger and the judged—that each man among us sits in the dock, as well as at the prosecutor's table. Only so can the necessary charity of which Bacon once spoke be compounded with knowledge. "Make," he said, "the time to come the disciple of the time past and not the servant." The words ring with such axiomatic brevity that without reflection the sharp ax-blade of his thought glances aside from our dull heads. Yet in that single phrase is contained the essence of Bacon, the educator, who, though trained in the profession of law, admonished posterity, "Trust not to your laws for correcting the times, but give all strength to good education." No man in the long history of thought strove harder to lay his hands upon the future for the sake of unborn generations. "I confess," he wrote, "that I have as vast contemplative ends as I have moderate civil ends."

His Life

Like the lonely African witch doctor recently described by Victor Turner (*2*), Bacon's whole life was spent in a barbaric society exposed to the dangers that attended those who surrounded the royal person. The headsman's ax was a familiar sight to him. He lived in its shadow. Gibbets were the seamarks for coasting mariners. Bacon had seen the heart torn from a living man leap after it was flung upon the fire. "It was an age," says one historian, "when it was

The author is provost of the University of Pennsylvania, Philadelphia. This article is based upon an address given at the Commemoration of the 400th Anniversary of the Birth of Francis Bacon, on 23 January 1961, under the joint sponsorship of the University of Pennsylvania and the American Philosophical Society. An expanded version delivered as "Francis Bacon and the modern dilemma" in the Montgomery lecture series at the University of Nebraska, 20–22 March, will be published by the University of Nebraska Press.

present drowning not to swim with the stream." With no means but that derived from royal favor, Bacon survived Elizabeth only to fall victim to James, the First. After achieving the office of Lord Chancellor, he ended his days in disgrace under charges concocted to make him a scapegoat for the crimes of his masters. If some of the mud of that harsh era in which he struggled to survive sticks to his doublet, let us remember with Blake the price of experience:

> Do men buy it for a song?
> No, it is bought with the price
> of all that a man hath, his house, his wife, his children,
> Wisdom is sold in the desolate market where none come to buy.

"There is nothing more lamentable in the annals of mankind," wrote Basil Montagu, one of Bacon's biographers, "than that false position, which placed one of the greatest minds England ever possessed at the mercy of a mean king and a base court favorite." It is ironic that those who took his possessions and destroyed his name are remembered only as shabby villains posturing eternally upon a stage from which their victim has long since departed.

James I—even Elizabeth—could not comprehend the world of which their courtier spoke, yet what he sought was no more nor less than an education which would give men power over their own destiny. "For the world," Bacon said, "is not to be narrowed till it will go into the understanding (which has been done hitherto), but the understanding to be expanded and opened till it can take in the image of the world. . . . Then, and only then," admonished Bacon, "shall we no longer be kept dancing within little rings, like persons bewitched, but our range and circuit will be as wide as the compass of the world."

It is customary to assume that the education of our day supplies all that Bacon envisaged, but a careful reading of his work will raise some doubt about the validity of this judgment. It may well be that we are still dancing in our own particular ring—a dance engendered, it may be, by some of his writings, but lacking the balance of his final judgment.

Like most classics, Bacon is more written about than read. Moreover, many who come to him for a phrase or a literary passage forget that he wrote both in a time of royal censorship and in a time when it was necessary to persuade men to follow new paths. For this reason his meanings are sometimes veiled and his appeals are made on a frankly material basis more frequently than might have been necessary at a later epoch. One has to read him all, and to know, at the same time, something of the despotism, "the little ring," that kept him chained. If it had been possible for him to gain the intelligent interest of the rulers he served, the march toward the modern world might have begun a century earlier—and perhaps (who knows?) under more balanced circumstances.

But it was not to be. All his eloquence gained him, in his generation, no more than the lonely and intelligent witch doctor of a small tribe might garner by spells in a dark forest. He died alone, in winter, coughing out his life, a stranger in the huge cold bed of a nobleman in whose house he had sought shelter. It is doubtful if in his dying hours this seemed to him in any way unusual. It was no more than sym-

bolic of the life he had led for all of his 65 years. The age had always been cold, and he had walked, reserved and wary, by rack and ax and gibbet to this final ending, out of favor and forgotten; no record remains of his funeral. "I leave my name . . .," he wrote with the sure instinct of one who knew that books would carry him like a wind-wafted ship safe out of the age in which he was imprisoned. "I leave my name [my knowledge, he might have added] in books exempted from the wrongs of time and capable of perpetual renovation."

New Continents

Total originality in anything so sweeping as a new philosophy is difficult, if not impossible, to demonstrate. Many scholars seem to believe that if they can show that Bacon did not originate induction, or that, if he talked much about inventions (*3*) but failed to make any, they will have succeeded in dismissing him as a leading figure of the scientific revolution. Actually, though Bacon talked about the experimental method, he was primarily an educator—one of the first in the new field of science.

He knew this very well and was humble about his own contributions. One thing he knew, however, with intense certainty: the world was changing. There was a wind blowing, not alone from the new continents discovered by the voyagers, but also from some new continent of the mind—some isle awaiting its birth in time. Bacon, with an almost supernatural prescience, had sensed the first faint breath of wind out of the future: "Let it be believed, and appeal made thereof to Time . . . that the New Found world of land was not greater addition to the ancient continent than there remaineth at this day a world of inventions and sciences unknown."

If men, in the past, have been impressed by some one discovery or invention, he reasoned, how much higher a thing "to discover that by means of which all things else shall be discovered." Pausing on the threshold of modern science he recognized that the scientific method itself is the invention of inventions. "The very beholding of the light," he tells us, "is a fairer thing than all the uses of it."

"But," counters the cynic, "his own scheme of induction is not original, nor does it work. Scientific discoveries are not made in this fashion, not by schemes, not by inductive ladders of ascent, but by the intuitions and insights of men of genius."

It is perhaps well at this point to examine just how seriously Bacon took his inductive system—a system which, it must also be remembered, he never completed. The originality, first of all, does not lie in the system. It lies, as Benjamin Farrington has very ably pointed out, in its application (*4*).

Bacon was one of the first time-conscious moderns. Unlike the ancients, he was not interested in a logical system intended merely to enable men to argue consistently, or to contemplate the universe. Instead, Bacon was concerned with process and with time. He was intent upon bringing his figurative "new continent" out of the limbo of the future. The traditional logic, concerned largely with eternal ideas and definitions, had to be reoriented in an empirical direction, in, as he puts it, "the discovery of particulars." Moreover, the contemplation of the natural world was not enough. An artificial world could, through knowledge, be imposed upon the purely natural world. "By the agency of man," contends Bacon, in what was then a bold and novel interposition of the human into the natural universe, "a new aspect of things, a new universe comes into view."

Bacon makes a sharp distinction between nature at rest and nature tormented by the experimenter: "For like as man's disposition is never well-known till he be crossed, nor Proteus ever changed shapes until he was straightened and held fast, so the passages and variations of nature cannot appear so fully in the liberty of nature as in the trials and vexations of art."

The "Art of Discovery"

It is frequently reiterated that Bacon felt that his inductive method had done away with the necessity for genius, and that he was laboring under the fixed idea that the collection of facts would almost automatically lead to discoveries. Two things have obscured his real intentions. First, much of his work, because of the vicissitudes of his career and the enormity of the task he attempted, remains fragmentary and of variable and uncertain date. Second, his own reservations are lost sight of in the cult which has surrounded his name. The following remarks, for example, are as dispassionate and as far from fanatical intemperance as could possibly be imagined:

> I think my rules are true and useful. But I do not say that they are either perfect or absolutely indispensable. I do not mean to suggest that nothing can be done without them. On the contrary I think that if men had available a good Encyclopaedia of Nature and of Art and would work hard at it, remembering just two of my rules—first, to drop all preconceived notions and make a fresh start, and second to refrain for a while from trying to rise to the most general conclusions or even near to them—they would succeed without any other rules of induction by the light of their own intelligence in falling into my method of interpretation . . . . I must admit the possibility that the art of discovery itself will advance as discoveries advance.

He knew, moreover, that, in the search for truth there was a double ladder, "ascendant and descendant."

Archibald MacLeish, in a fine poem, that may well be familiar to numerous readers of *Science*, "Epistle To Be Left in the Earth," narrates the thoughts of a last, desperate survivor on a dying earth. "I pray you," the unknown survivor writes ("if any open this writing"),

> Make in your mouths the words that were our names.

Laboring under the heavy burden of his own mortality, he cries anxiously,

> I will tell you all we have learned
> I will tell you everything.

The poem then runs off into the disjointed efforts of a man scrawling, haphazard, a last few facts about this planet:

> It is colder now
> there are many stars
> we are drifting
> North by the Great Bear
> the leaves are falling
> the water is stone in the scooped rocks.

There is a poignant similarity between this verse and the real life creation of Bacon's *Sylva Sylvarum*. The book was written in haste in his declining years after all his hopes for new universities, co-workers in science, and aid from enlightened rulers had been disappointed. But the man, like the individual in MacLeish's poem, still struggled to collect the facts out of which the new continent should be built. He is aging, hope is gone, the task looms gigantic. He has no adequate conception of the size of the universe he has attempted to engage. No one

will come to his aid. The book becomes an almost incoherent babble of facts drawn both from personal observation and diverse sources. Moss grows on the north side of trees. Strange fungi spring up in the forests. Fruit put into bottles and lowered into wells will keep long. He writes of the moon and vinegar, of cuttle ink, and of the glowworm. These are such facts as each one of us, divested of four centuries of learning, might try to record for posterity if we were the last of a dying race.

In a sense Francis Bacon was such a man. He was dying seemingly without scientific issue; the great continuity of learning for which he pleaded had been received indifferently by the world. Yet hidden in the *Sylva Sylvarum,* regarded as of no importance today, is a quite remarkable statement.

The passage is striking because it sets the stage for as pure a demonstration of the value of the induction for which Bacon argued as he could possibly have hoped for. Yet because 250 years were to elapse in the reasoning process, men have forgotten the connection. After some observations upon changes in plants, he remarks: "The transmutation of species is, in the vulgar philosophy, pronounced impossible, and certainly it is a thing of difficulty, and requireth deep search into nature; but seeing there appear some manifest instances of it, the opinion of impossibility is to be rejected, and the means thereof to be found out." This tolerant and studious observation, with its evolutionary overtones, was made before the nature of fossils was properly understood and before the length of geological time had been appreciated.

"The path of science," Bacon had proclaimed, "is not such that only one man can tread it at a time. Especially in the collecting of data the work can first be distributed and then combined. Men will begin to understand their own strength only when instead of many of them doing the same things, one shall take charge of one thing and one of another."

For the next 200 years men allied in international societies originally foreseen by Bacon would make innumerable observations upon the strata of the earth, upon fossils, and upon animal and plant distributions. Heaps upon heaps of facts collected and combined by numerous workers would eventually lead to Darwin's great generalization. In the end Darwin himself was to write: "I worked upon the true principles of Baconian induction" (*5*). The individual empirical observations which led to the theory of evolution and the recognition of human antiquity had been wrenched piecemeal from the earth.

Bacon, moreover, was keenly cognizant of the value of the history of science. He deplored its neglect and urged that all such history of "oppositions, decays, depressions [and] removes . . . and other events concerning learning will make men wise. . . . For," continues Bacon, "the unlearned man knows not what it is to descend into himself, or to call himself to account . . . whereas with the learned man it fares otherwise, that he doth ever intermix the correction and amendment of his mind with . . . the employment thereof."

Bacon also anticipated the folly of great thinkers in their tendency to extrapolate too broadly from the base of a single discovery. Thus, though he admired Gilbert's discovery of the magnet, Bacon wrote: "he has himself become a magnet; that is he ascribed too many things to that force." One might observe that this well-known tendency is apparent in Louis Agassiz's final exaggeration of the extent of the Ice Age. Other equally pertinent examples could be cited. Even the great thinker, warns Bacon, sometimes shows a tendency to create anew a ring within which he dances.

Building with Common Clay

One other view which Bacon advocated was severely criticized by Macaulay in the early 19th century, and still evokes comment today—namely, his so-called neglect of genius. "The course I propose for the discovery of sciences," Bacon argues, "is such as leaves but little to the acuteness and strength of wits, but places all wits and understandings nearly on a level."

At first glance such a remark is apt to offend the superior intellect. In actuality, however, just as Clemenceau is reputed to have remarked that war was too important to be left to generals, so Bacon is not content to leave the development of the sciences to the sporadic appearances of genius. Nor, practically speaking, I think, would any educator today.

A careful reading of Bacon reveals that what he is anxious to achieve is the triumph of the experimental method. This triumph demands the thorough institutionalization of science at many levels of activity (*6*). In one passage he encompasses in a brief fashion all those levels on which science operates today. "I take it," he writes prophetically, "that all those things are to be held possible and performable, which may be done by some persons, though not by everyone; and which may be done by many together, though not by one alone; and which may be done in the succession of ages, though not in one man's life; and lastly, which may be done by public designation and expense, though not by private means and endeavor." Only in this manner can the continuity of the scientific tradition be maintained and the small bricks which go into the building of great edifices be successfully gathered. "Even fourth rate men," Darwin was later to observe, "I hold to be of very high importance at least in the case of science."

If Bacon meant anything at all he meant that working with the clay that sticks to common shoes was the only way to ensure the emergence of order and beauty from the misery of common life as his age knew it. He eliminated, in effect, reliance upon the rare elusive genius as a safe road into the future. It partook of too much risk and chance to rely upon such men alone. One must, instead, place one's hopes for Utopia in the education of plain Tom Jones and Dick Thickhead.

Ironically, this was the message of a very great genius, an aristocrat who had lived all his life in the pomp of circumstance, but who, in the end, was willing to leave his name to later ages and his work to their just judgment. "I say without any imposture, that I . . . frail in health, involved in civil studies, coming to the obscurest of all subjects without guide or light, have done enough, if I have constructed the machine itself and the fabric, though I may not have employed or moved it."

Bacon had an enormous trust in the capacities of the human mind, even though no one had defined better than he its idols and distortions. "There be nothing in the globe of matter," he wrote, "that has not its parallel in the globe of crystal or the understanding." John Locke, almost a century later, is far more timid than this. Perhaps Bacon reposed too much hope in the common man. Or perhaps it is we who lack hope, the age for which Bacon waited being still far off, or a dream. But is it not a very great wonder that a man who spent all his life in the arrogant class-

conscious court of a brutal age strove for personal power as a means of transmitting to the future an art which would, in a sense, make him, a very great genius, and men like him, almost unneeded for human advancement?

I know of no similar event in all history. As an educator in a country which has placed its faith in the common man, I can only say that the serenity of Bacon's faith takes our breath away and gives him, at the same time, our hearts. For he, the Lord Chancellor, was willing to build his empire of hope from common clay—from men such as you and I. "It is not," he protests, "the pleasure of curiosity . . . nor the raising of the spirit, nor victory of wit, nor lucre of profession, nor ambition of honor or fame, nor inablement for business, that are the true ends of knowledge." Rather it is "a restitution and reinvesting of man to the sovereignty and power which he had in the first state of creation."

Have we, Bacon's intellectual descendants, forgotten this vision? Are we swollen with that venomous sting of which he warned us, as contained in knowledge without charity? Are we already about the business of man's second fall?

The Light of Reason

I shall here reiterate that I have taken the case for Bacon's defense, not to confuse the jury, or to conceal crimes, or to claim innocence, but because I am part, as all men are part, of this venture and because each of us sits in the dock with him and knows that after 400 years judgment is not yet passed, nor can it yet be passed, upon him and the work he did. For is it not written in a certain Book that a thousand years are but as a day?

Those who dismiss Bacon as a scientist because he made no inventions have forgotten his own uncanny and preternatural answer, for he remarked that in the beginning there was only light. The grinding of machines and the sounds above us in the air we have taken for the scientific fruits he spoke of. After 400 years the light, however, is only along the horizon—that beautiful, dry light of reason, which Bacon admired above all things, and which he spoke of as containing charity. Has the prisoner before the bar seen further still? Is he talking, still, to an age farther away and more remote from us than Elizabeth's court will ever be?

The defense rests. It does not ask that the prisoner be discharged, but that he be reprieved. I who sit in the dock and yet argue this case before you plead for another thousand years.

Ours is not the light he saw. Ours is still the vague and murky morning of humanity. He left his name, the name of all of us, to the charity of foreigners and the next ages. We presume if we think we are those addressed in his will. We are, instead, only a weary renewed version of the court he knew and the days he wore out in blackness. The gyrocompass in the warhead has no new motive behind it; the Elizabethan intrigues that flung up men of power and destroyed them have a too-familiar look; the religious massacres that shook Bacon's century have only a different name in ours.

There is something particularly touching about Bacon's growing concern "to make the mind of man, by help of art, a match for the nature of things." He knew, in this connection, that man the predator is also part of that nature man had to conquer in order to survive. Bacon had sat long in high places; he knew well men's lusts and rapacities. He knew them in the full violence of a barbaric age.

Although he has been accused of giving "good advice for Satan's kingdom," he understood from the beginning, and stated in no uncertain terms, that the technological arts "have an ambiguous or double use, and serve as well to promote as to prevent mischief and destruction, so that their virtue almost destroys or unwinds itself. . . . All natural bodies," he contended, with some dim, evolutionary foreboding, "have really two faces, a superior and inferior." Man has in him "something of the brute."

In one of those strange yet powerful sentences which project like reefs out of the sum total of his work, he gives us a Delphic prophecy: "Whatever vast and unusual swells may be raised in nature," he says, "as in the sea, the clouds, the earth or the like," so that in this age our mind flies immediately to man, "yet nature," he continues, "catches, entangles and holds all such outrages and insurrections in her inextricable net, wove as it were of adamant."

John Locke, some decades later, struck by the immensity of the great American forest, cried out, "In the beginning the whole world was like America." Had Bacon, spokesman for science, a hundred years earlier, seen the possibility of the return of that forest even before it had departed? Or did he look beyond this age to a time when, by greater art than now we practice, we may have made our peace with the nature of things?

"It must ever be kept in mind," Bacon urged, "that experiments of Light are more to be sought after than experiments of Fruit." The man was obsessed by light—that pure light of the first morning of Creation before the making of things had commenced, before there was a garden and a serpent and a Fall, before there was strontium and the shadow of the mushroom cloud. He who will not attend to things like these can, in Bacon's own words, "neither win the kingdom of nature nor govern it."

Because Bacon saw and understood this light, it is well, I think, that he be not judged by us. Those who charge him, like a necromancer, with having called up from the deeps of time the direst features of the modern age, should ponder well his views upon the soul—"the world being in proportion," as he says, "inferior."

"By reason whereof," our worldly philosopher adds, "there is agreeable to the spirit of man, a more ample greatness, a more exact goodness, and a more absolute variety, than can be found in the nature of things." Because he could not renounce this vision, in the name of man, in the name of all of us, the defense of Francis Bacon rests—for another thousand years.

References and Notes

1. In simple justice it should be said that much of this widespread impression springs from the biased and unfair but widely circulated essay of Lord Macaulay. The student would do well to study the defense of Bacon contained in the lifelong researches of James Spedding, H. Dixon, Basil Montagu, Bryan Bevan, J. G. Crowther, and others who have exhaustively reviewed the evidence and exploded many of the false assumptions upon which estimates of Bacon's character have been based.
2. V. Turner, in *In the Company of Man*, J. B. Casagrande, Ed. (Harper, New York, 1960).
3. It must be remembered that in Elizabethan times the word *invention* applied to ideas and the arts of literature as well as to mechanical devices.
4. B. Farrington, *Francis Bacon: Philosopher of Industrial Science* (Schuman, New York, 1949).
5. Darwin's sincerity in making this statement has been questioned, but for our purpose the facts are sufficient. Whether consciously or not, Darwin and his forerunners had assimilated the observations of previous observers in a long process of induction.
6. John Storck has commented that "at this point, if no other, Bacon contributed an idea of the first importance to the development of modern science" [*J. Philosophy* **28**, 183 (1931)].

Keys to Cultural Understanding

Figures of speech are tangible manifestations of fundamental modes of perception and conceptualization.

Roland W. Force and Maryanne Force

The ability to communicate through the use of language is peculiar to mankind (*1*). One of the simplest definitions of any language is that it is a system of arbitrary vocal symbols by means of which members of a social group cooperate and interact. Anthropologists view language as the basis for all cultural activities (*2*). Linguistic behavior is of course but one kind of human behavior—a most significant kind for analysis, however, since man in all his social relations depends on the use of language. Though the meanings of the arbitrary symbols that form any language are considered important by linguists, the basic concern of linguistic science in America has been with the structure and relations of linguistic symbols and with historical linguistics. The area not so well investigated is that of the relationship of language to culture (*2*).

The chief architect of this latter variety of linguistic study was Edward Sapir. He viewed language as a guide to social reality and held that human perception is conditioned basically by language. To him, even "simple acts of perception are very much more at the mercy of . . . words than we might suppose" (*3*). The work of Benjamin Lee Whorf on the behavioral effects of modes of perception and conceptualization reflects Sapir's influence. According to Whorf, the systems of grammatical structure in a language determine what the individual perceives, or can perceive; how he thinks, or can think, about his world. Language functions not only as a device for reporting human experience but also as a way of defining, analyzing, and organizing it through prescribed channels into meaningful categories for its speakers (*4*). Linguistics, said Whorf, is essentially the "quest of MEANING . . . a heuristic approach to problems . . . a glass through which . . . will appear the TRUE SHAPES of many of those forces which hitherto have been . . . but the inscrutable blank of invisible and bodiless thought" (*5*).

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Communication and Meaning

In the ever-constant process of communication people call upon the familiar expressions of their language—expressions which they have used in the past—sometimes attaching new meanings either consciously or unconsciously. Comparisons are made and analogies drawn. One thing is likened to another. In our own culture, for example, an intelligent person is said metaphorically to be "brilliant," respectable citizens are referred to as "pillars" of society (these individuals are the so-called "solid" citizens), and cunning behavior becomes "foxy." The names for various flowers provide examples of metaphor in abundance. The term *daisy* is properly *day's eye*, and was applied first to the sun and secondarily to the flower; larkspur, cockscomb, and even tulip and iris may all be traced to something considered similar by some innovator.

Metaphors commonly are based upon relativity of form, function, color, position (elevation, distance, in-ness or out-ness), proportion (size or weight), viability, resilience, validity, and so on. Generally there is a tendency to use "concrete" terms for abstract ideas, such as may be noted in proverbs, parables, and sayings. Similarly, through synecdoche, meanings related to parts expand to relate to wholes. Workmen become *hands*, an intelligent person is called a *brain*, and livestock numbers are designated by so many *head*. In parts of Malaya animals are tallied not by their heads but by their tails—so many tail of cattle. This practice extends even to the seemingly illogical custom of referring to frogs and other tailless creatures in similar fashion.

In metonymy one word is used for another with which its meaning is closely connected. The restaurant (or, ludicrously, the airplane or steamship) has an excellent *cellar*. We read *Whitehead, Virgil,* or *Shakespeare*. Examples from contemporary English usage could be added almost infinitely (*6*). The exercise need not be limited to English, for figures of speech are found in all languages. Philologists and linguists concerned with etymology explore such modes of expression with rich rewards. "If we would discover the little backstairs door that for any age serves as the secret entranceway to knowledge, we will do well to look for certain unobtrusive words . . . which, having from constant repetition lost their metaphorical significance, are unconsciously mistaken for objective realities" (*7*).

It has been pointed out by students of language that most languages actually operate with a rather limited lexicon. Basic vocabularies in common use differ in their extent to be sure, but none fully exhaust the potential of the language. In any language some words have multiple meanings. However, speakers, guided by context, usually are not troubled by semantic variations. Innovations which alter lexical meanings but do not change grammatical function are called *semantic* changes by linguists. Figures of speech, so innovated, are later transmitted to other speakers and achieve currency.

Home-Made Models

An analysis of the use of figures of speech in a given culture (insofar as they pertain to human relations) may provide insights unachievable through other modes of analysis, help to corroborate other findings, or both. Such an approach can serve as a tool for opening "the way to a broader analysis

which considers all the ways of organizing the world around us and the ways this is reflected in our social organization and the other parts of culture" (*8*). Figures of speech act as models which the people of a culture construct, sometimes consciously and sometimes unconsciously, to illustrate and define both social and nonsocial relationships. They may be termed the "home-made" (*9*) models which help people to define their universe and its workings, and which enable them to render the abstract concrete. The study of social organization deals with abstract relationships almost exclusively—relationships between individuals and between local and kin groups. In view of the prevalence of such relationships and the need to express abstract ideas, we should expect a widespread use of figures of speech in reference to societal organization. This situation seems to hold especially true in the Palau Islands of Micronesia (see Fig. 1). It is our purpose in this article to cite some examples of the use of figures of speech in Palau and to illustrate their significance in the general area of social organization (*10*). We occasionally cite comparable data from other cultures.

Fig. 1. Map showing the location of the Palau Islands.

In Palau, as elsewhere, the fact that an utterance is actually a figure of speech is often overlooked by the speaker. The usage may be so accepted as a matter of fact and convention that even when the figure is pointed out, the speaker may not be convinced easily. A case in point has to do with the Palauan words for "five" and "hand." The terms are often cognate in Malayo-Polynesian dialects, as they are in Palau. There the term for "my hand" is literally "my five," referring, of course, to the five fingers. Other figures are more easily recognized by the person using the expression. For example, the little finger is called "child," obviously because of its diminutive size. In contrast, the middle finger is considered relatively more important and has the same name as that used to designate descent in the female line (*ochel*). Though features of bilateral organization are present in Palau, maternal descent is emphasized over paternal descent.

Another example of a term with accepted meanings being applied in another context is seen in the forming of the superlative in Palau. Maternal descent, as just mentioned, is emphasized in preference to paternal descent, though both are reckoned. Senior females in prestigeful kin groups, considered generally to be of high status, and women of advanced age are universally called "mother," whether or not there is a genealogical connection between them and the speaker. The positive values which accrue to the term for mother are readily seen in superlative expressions. Largest or oldest is *delal a klou* (literally, "mother of large or old"); best is *delal a ungil* (literally, "mother of good"); and highest is *delal a ngarabub* (literally, "mother of up"). This usage reflects an important organizational bias.

Natural Prototypes

To Palauans a beautiful woman is a "comet." An illegitimate child is a "child of the woods or bush." The term for ocean current (*omtelub*) has come to be used to describe repetitive accusations. A Palauan born in a given village is said to be the "meat" of a certain nut (*techel a miich*) of that village. This expression illustrates one's extremely close relationship to his place of birth. The various developmental stages of a coconut also provide material for several figures of speech. One of the most notable of these is an honorific designation for elder females, who are called *mechas* (literally, "mature coconut"). In Hawaii a man without male siblings is called "a single coconut" (*11*). To Palauans, the first child is *ketingek* (literally, "my first child") from the expression *ketingel a du* (literally, "first fruit of the banana tree"). In contrast, the youngest child is called the "tail of the fish."

Figures of speech are used in Palau to refer to concepts of considerable magnitude as well as to those of relatively small proportions. The two territorial and political divisions of the islands in aboriginal times were referred to as *bital eiyanged ma bital eiyanged* ("other heaven or sky and other heaven or sky"). Often remarked upon in discussions of Palauan societal organization is the dual character seen in this last example. The presence of a dual system of organization can be seen throughout the social structure. Whole villages in aboriginal times were visualized as having a "right" and "left" side; *bital belu ma bital belu* (literally, "other village and other village"). This division was called "the split of the village" (*metiud a belu*). Corresponding to the scheme of village division in Palau was an institutionalized division of village age-grade societies. Theoretically, in each half of the village there were three men's clubhouses. Each half had its own water channel (*taoch*), which was used in the launching and landing of watercraft from that moiety. The division of the age-grade societies was conceived analogically; each group of three clubhouses was called a *taoch*. Just as the village was divided into two parts (*bital belu ma bital belu*), age-grade societies were

divided into *bital taoch ma bital taoch* ("other channel and other channel").

Figures of speech are sometimes used in reference to broad segments of a kinship system. Pehrson (*12*) has noted that the Könkämä Lapps use them in this way when they speak of a tree with its ramifying branches and root system to refer to kindred. On the other hand the reference may be relatively limited, as in Palau, where the complete analogy with a tree is not drawn, but where relatives separated by spatial distance (not by genealogical distance) are referred to by the term which denotes the tip of a tree branch. Similarly, the beginning (origin) of a sib is called by the same term as is used to refer to the stump (origin) of a tree. The term is usually applied to the earliest traceable ancestral female within a matrilineage. Such a person is also called "mother of the turmeric plant." This expression may be a combination of the use of the term *mother* in the superlative sense and a reference to the important turmeric plant as a kinship model.

The turmeric plant also serves as a model when a man wishes to designate his sister's children and the children of female nieces (his sister's daughter's children). These he calls by terms which refer to the roots of the turmeric plant (see Fig. 2). Such roots consist of tubers which are progressively farther away from the rhizome. A man calls his sister's children by the same term which is used to designate the proximal

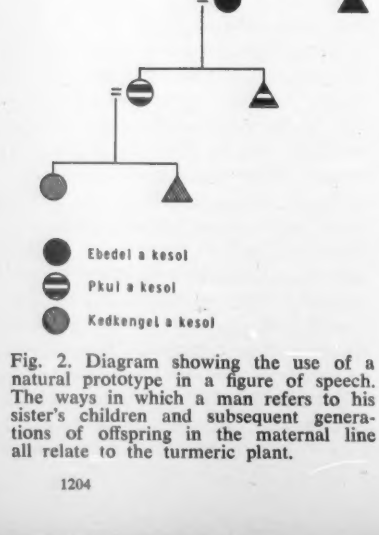

Fig. 2. Diagram showing the use of a natural prototype in a figure of speech. The ways in which a man refers to his sister's children and subsequent generations of offspring in the maternal line all relate to the turmeric plant.

	Nuclear Family	Extended Family	Lineage	Sub-sib	Sib	Super-sib
NATIVE TERM:						
oungalek	x					
talungalek	x	x	x	x	x	
keblil		x	x	x	x	(x)[1]
kleblil					x	x
blai	x	x	x	x	x	x

[1] occasionally

Fig. 3. Kin group terminology overlap in Palau. The terms *keblil* and *kleblil* are derivatives of the generic term for house (*blai*).

tuber, *ebedel a kesol* (literally, "close to the turmeric plant"), whereas a sister's female child's children are called by the name for the next nearest tuber, *pkul a kesol* (literally, "projection of the turmeric plant"). Offspring of the next descending generation in the maternal line are called *kedkengel a kesol*. The term *kedkengel* means "a new shoot" and is customarily used to designate a new shoot of taro.

Among the Kuma of highland New Guinea, references to an edible leaf symbolize kin relationships. Close kinsmen, such as brothers (same mother), are said to eat the same leaf. In recognition of cultural unity the same thing is said of less closely related and larger social aggregates. Alien groups or tribes are considered a different kind of people, since their edible leaf is not the same (*13*).

As noted above, figures of speech which make reference to body parts can provide additional significant cultural insights. In a manner similar to English usage, a village or sib leader in Palau is called the head (*ptelul*) of the village or sib. Moiety arrangements within kin groups are compared with legs because of the analogy with bifurcation. Groups of people within a lineage, sib, or super-sib who are assumed to be related by virtue of the fact that they are descendants of the members of certain ancient migrations are referred to as *bital wa ma bital wa* ("other leg and other leg"). This expression is applied as well to the descendants of two individuals who were closely connected, such as sisters by adoption, and to two kin groups which have been fused into a larger one. Descendants of relatively late migrations are referred to discriminatingly by Palauans as "wet legs." The reference defines those it is directed toward as having arrived so recently (though the migration may have been generations before) that their legs are still wet from having waded ashore.

An extremely close kin relationship such as that between the children of two sisters is referred to as "other stomach and other stomach—just one person." This reference recalls one found on Yap, where the kinship term *ngayil* refers to siblings born of the same mother and "is a contraction of two words meaning 'same belly' " (*14*). Commenting on the Chinese kinship system, Kroeber has remarked upon the presence of metaphorical supplements or qualifiers. An example similar to those noted for Palau and Yap concerns the use of the term *placenta* to denote "own brothers or sisters"—that is, siblings as distinct from cousins (*15*).

Relatively weak consanguineal ties in Palau are called *ngamkehui* ("a hair of the smallest diameter"). Figurative references to lineal descent are equally logical and, in at least one case, euphemistic. When referring to descent in the maternal line, the word for stomach (*delach*) is applied, whereas paternal descent is designated by the word for blood (*rasech*), which is a euphemism for semen (*tiied*). A final example of the use of a figure of speech having to do with body parts and social organization is the use of the word for heel (literally, "your first") to signify one's place of birth. Thus "your heel" and "your place of birth" are one and the same.

Cultural Prototypes

An intriguing aspect of Palauan societal organization is the extension through metonymy of the term for house (the physical structure) to refer to the group of individuals dwelling

therein. This may be either a nuclear family or an extended family. The term *blai* (literally, "house") by similar logic also is used to refer to larger kin groups whose members do not live together under one roof but who are related or are assumed to be related consanguineally. Thus, lineages, sibs, and even the more nebulous super-sibs are called *blai* as well (see Fig. 3).

Firth has noted a similar "predilection for using a word with a wide variation in meaning in different contexts" in Tikopia (*16*). He notes that the Tikopian word for house is *paito*. Through a transfer of meaning, the term not only refers to the family living in a given house but is the "recognized designation for the kinship unit constituted by a number of households living under different roofs" (*16*). *Paito*, then, are not necessarily localized residence units; they may be scattered through several villages. What is demonstrated here, and in Palau where a similar situation prevails, is that the relation between the physical structure and the kin group associated with it has been either consciously or unconsciously recognized and an appropriate term has been employed through the process of metonymy. In Palau the generic term for house (*blai*) is related (as *paito* must be) to the Indonesian root term for house (*balay*) (*17*). Through the addition of a prefix connoting mutuality and through conjugation for tense, the Palauans have developed several related terms for different levels of kin group membership, all of which may be traced back to the generic term for house.

Another usage which has its basis in the analogy between a house and a kin group has to do with incest regulations. When one makes a money payment in native bead currency to rectify incestuous behavior it is called "making the floor clean." Obviously, this is a euphemism referring symbolically to the "floor" of the house (kin group). The meaning is clear to Palauans, since close genealogical relationships are customarily indicated by saying that the individuals involved are all "people of one house" or that they are "people of one floor."

Aboriginally, one of the most distinctive architectural structures in the Palaus was the *abai* (council house or clubhouse) (see Fig. 4). Every village possessed several such structures, each of which was the result of considerable expenditure, physical labor, and artistic endeavor. Consequently these structures were very much valued and were maintained for many years, even after structural timbers had begun to sag with age. When an aged *abai* was in danger of collapse, owing to the weakness of its main support members, the custom was to provide external props of wood all around the structure. These angled from the upper sidewalls near the eaves to the ground. Analogously, a parallel between the *abai* and a kin group (sib or lineage) was drawn. The leader (senior male in the maternal line) of a kin group was compared to these supports. It was he who was viewed as the major support of the kin group and thus was referred to as the *tkakl a keblil* ("support of the kin group"), just as a wooden prop was called *tkakl a bai* ("support of the council house or clubhouse").

Seligman (*18*) calls attention to the practice of associating certain kin groups and title-holders with the posts of the *dubu* platforms and the steeple houses of Papua. He mentions the practice of dividing a structure into "front" and "back" halves and the relation of various clans with the halves and the structural posts they contained. This practice also occurred in aboriginal Palau, where council houses were divided into front (*ngelong*) and back (*rebai*) halves. Seating arrangements in front of the posts reflected the moiety alignment of the village sibs.

Within Palauan villages there exists a system of ranked sibs (unilinear consanguineal kin groups). The leaders and members of the four highest-ranking sibs are the elite. Traditionally, their will was the law of the village and they dominated all activities. These four highest-ranking sibs are called the *kloal saos* ("four posts"). The reference is to the four corner posts of a house or council house, which support the building. In like manner the four

Fig. 4. A contemporary council house in Ibukl village of Ngerechelong municipality in the Palau Islands.

top-most sibs support the village structure. The role of the sibs is aptly described metaphorically. In Samoa there is a comparable relationship between the two highest chiefs present at a meeting and the two main posts at either end of a round house. The posts serve as back rests, and at the same time they signify relative status. Other architecturally less significant posts serve similarly for lesser-ranking chiefs (*19*).

A common practice in Palau is that of utilizing terms usually employed in the designation of kinship (parent-child or sibling-sibling) to refer to villages considered to have especially close ties. Thus, a particular village is a "child" (*ngelegel*) or younger sibling (*chocholel*) of another village. There is a somewhat similar usage among the Mountain Arapesh of New Guinea. Beach villages are referred to as "mother villages," and the mountain villages are called their "daughters" (*20*).

Franz Boas remarked that to him it seemed that the relationship terms by which some of the Indian tribes of eastern North America designate one another were originally "nothing but a metaphorical use of these terms, and that the further elaboration of the social relations of the tribe may have been largely determined by transferring the ideas accompanying these terms into practice" (*21*).

In Palau the word for antagonist (*chesmachel*) is applied to certain male affinals referentially. A man will refer to his son-in-law (his daughter's husband) as *chesmechelek* (literally, "my antagonist"). The close connection between the methods of denoting relationship or kinship and forms of social organization has been noted many times since Rivers first pointed it out (*22*). Such a connection exists in Palau, where consanguineal ties are emphasized over and above affinal ones. It is not surprising, then, to find the term *antagonist* used in reference to certain affinals. What is seen is another case of the drawing of meanings from one context and their application with a common term to another.

Just as one figure of speech may emphasize the absence of close relationship (or even antagonism, latent or overt), another may point to its presence. The term *kaubekl* refers to a married couple and signifies their close conjugal relationship; it also refers to a type of canoe where the oarsmen are seated in pairs.

Summary

Figures of speech are tangible manifestations of deeply rooted modes of perception and conceptualization. They represent categories or models for interpreting the universe—natural and supernatural, and material and nonmaterial phenomena. These categories or models are reflected in all aspects of culture and are fundamentally determined by culture. They should not be considered universal but, rather, extremely relative and highly variable. Some students of language and culture, such as Whorf, would consider figures of speech not to be the most important organizing categories and would give precedence to grammatical constructions—that is, subject-predicate division or verb tenses. The use of figures of speech, however, is in some ways more easily investigated than grammatical categories, and their analysis can provide both unique and corroborative insights into various aspects of a culture, as has been demonstrated here with Palauan social organization.

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Scientist and Public

Why is the scientist, once a "natural philosopher," now considered a barbarian by many educated laymen?

René Dubos

Let me say, by way of introduction, that I do not have the professional knowledge of the historian, nor have I ever engaged in true historical research. The most I can claim is a long and continued interest in the *results* of historical scholarship, and a willingness to use them for guidance in some of my scientific studies. Thus, I speak not as a contributor to the history of science but, rather, as a consumer and, at best, a devotee.

A report published last July in *Science* [**132**, 68 (1960)] provides a striking illustration of the relevance of historical knowledge to the social attitude of the scientist in the modern world. *Science* devoted five pages to the report of the AAAS Committee on Science in the Promotion of Human Welfare. "Having become a major instrument in political affairs," the report states, "science is inseparably bound up with many troublesome questions of public policy. That science is valued more for these uses than for its fundamental purpose—the free inquiry into nature—leads to pressures which have begun *to threaten the integrity of science itself*" (italics mine). It is very timely to consider this statement in its historical context because the issues that it raises can be traced to the writings of a man whose quadricentennial we are celebrating this year—namely, to Francis Bacon (1561–1626), who has been called the first statesman of science.

Bacon

So much has already been written concerning Sir Francis Bacon, Lord of Verulam, Grand Chancellor of England—still assumed by some admirers to be the real Shakespeare—that I need mention here only the particular aspects of his role as prophet of modern science which have a direct bearing on the present topic. Bacon's own contributions as a scientist were trivial, and whatever the merit of the inductive method which he advocated, it was not original with him. Yet, his name deserves to survive in scientific history because, through the vigor of his convictions and the splendor of his language, he became the symbol of the belief that science is an infallible instrument to solve practical problems. Scientific knowledge, he claimed, would permit man to recapture his dominion over nature and thus to regain the happiness that was Adam's before the Fall. Furthermore, Bacon helped spread the doctrine, then unorthodox, that to improve the lot of man on earth is the real justification of science. In his words, science is not "for pleasure of mind . . . but for the benefit and use of life"; its "true and lawful goal . . . is none other than this: that human life be endowed with new discoveries and powers."

Bacon was not, of course, the first one to have become aware of the practical potentialities of science. He was but the eloquent voice of the many forces that converted Europe from the scholastic acceptance characteristic of the Middle Ages to the dynamic attitude associated with the Renaissance and the Reformation. But granted that he acted chiefly as the voice of his age, he had a creative influence, nevertheless, through helping Europeans to realize the social power of science. He acted as a gadfly, and sounded a call to organize life for the scientific era. In his words, he "rang the bells which called the wits together."

Not only did Bacon preach that science is power, he also imagined in his Utopian *New Atlantis* the first model of a society based on scientific wisdom, a sort of blueprint for the modern world. The measure of his originality is provided by the fact that no other creator of Utopia has described anything comparable to the community of scientific scholars that constituted "Salomon's House," the ruling body of "New Atlantis." His imaginary commonwealth was organized to make use of technology in every department.

A New View of Knowledge

It is certain that, directly or indirectly, Bacon's description of "Salomon's House" influenced the organizers of the scientific academies that sprang up during the 17th and 18th centuries. The emphasis on the practical aspects of science appears, for example, in a letter by Robert Boyle, who was one of the first members of the "invisible college" which eventually became the Royal Society in 1660. In this letter Boyle stated that he had been studying "natural philosophy, the mechanics, and husbandry, according to the principles of our new philosophical college, that values no knowledge, *but as it hath a tendency to use*" (italics mine). And Bishop Thomas Sprat, the author of the first history of the Royal Society, published in 1667, referred to Bacon with the statement that "there should have been no other preface . . . but some of his Writings." The French Encyclopedists also acknowledged their indebtedness "to the immortal Francis Bacon . . . to consider the just and extensive Views of this prodigious Man; the Multiplicity of his Objects; the Strength of his Style; his sublime Imagery; and extreme Exactness; we are tempted to esteem him the greatest, the most universal and most eloquent of all Philosophers. . . . It is to this great Author we are chiefly indebted for our Encyclopaedic Plan."

Thus Bacon symbolizes, even though he obviously did not initiate, a very profound change in the attitude of man toward knowledge. In the past, science had been primarily concerned with the search for law and order. It was

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theoretical rather than practical—an adornment of life. After Bacon, knowledge increasingly became an instrument to achieve mastery over nature. First in the Western countries, then all over the world, this attitude became identified with progress and civilization. The statement attributed to Karl Marx applies well to scientists in all modern societies: "Hitherto philosophers have sought to understand the world—henceforth they must seek to change it."

Science and Public Policy

At this point I might be expected to discuss the manner in which the ever-increasing role of science is presently affecting its support and organization. And indeed many perplexing questions are arising at the point of contact between science and society. They concern the policy makers of science, the scientists who produce it, and the public who uses it—in other words, all of us. Most scientists believe that science progresses best under conditions of absolute freedom. And yet, can scientists really hope to retain freedom in the selection of their problems, in the conduct of their studies, and in the applications of their findings, now that they are in the process of becoming public servants? Is it not likely that politicians and even statesmen will increasingly adopt toward science and scientists the attitude symbolized by Clémenceau's opinion of soldiers—"War is too serious a business to be entrusted to generals"? Important as these problems are, I shall not consider them here, because I want to focus my attention on another aspect of the interplay between scientists and public which is rarely discussed—namely, the cultivation of science for knowledge's sake.

Improvement of Man's State

Not only did Bacon urge that the proper object of science is the improvement of man's state, he even went so far as to suggest that there is something almost sinful in the use of science for purposes other than the practical. "Knowledge, that tendeth but to satisfaction," he wrote, "is but as a courtesan, which is for pleasure, and not for fruit or generation."

The attitude toward science implied in the preceding quotation is not uncommon among practicing scientists today, and indeed it conditions their views on the "social implications" of their professional activities. In the usual discourse, these implications refer almost exclusively to the practical uses of scientific knowledge, little to its philosophical relevance. Two stories, concerning Faraday and Pasteur, illustrate the extent to which this attitude affects the public behavior of scientists. It is told that Faraday once received the visit of an important person in his laboratory at the Royal Institution, shortly after he had discovered electromagnetic induction. He gave a demonstration of the phenomenon to his visitor, who was apparently unimpressed and inquired, "What is the good of this discovery?" To which Faraday is alleged to have replied, "Someday, Sir, you will collect taxes from it." Pasteur also used the lure of practical applications in undertaking to convince his students of the importance of scientific studies. At the University of Lille, in 1856, he recounted that Franklin, on being asked about the use of a certain discovery, merely replied, "What is the use of a new born baby?"

The statements attributed to Franklin, Faraday, and Pasteur imply, of course, an awareness of the social power of experimental science. The answer, "What is the use of a new born baby?" clearly symbolizes the faith that scientific discoveries are not an end unto themselves, that they hold in potential many further developments, not all of which can be readily predicted because so much depends on future circumstances. And the suggestion that taxes will someday be collected from a new scientific fact is but a more concrete and worldly statement of the same faith. It expresses the belief that almost any scientific discovery will eventually be converted into some process or product which society can use, and for which it is willing to pay. But Franklin, Faraday, and Pasteur did not tell the whole truth, because they did not express completely their own attitude toward science.

Faraday

Faraday was an active member of a very strict sect of the Nonconformist Church. His lofty religious ideals and his urge to pursue as far as possible the spiritual implications of his scientific work led him to abandon all industrial consultation work and to renounce the monetary and social advantages that he could have derived from his immense fame. Yet, while he properly emphasized the usefulness of science in the practical affairs of man, he rarely voiced his inner conviction that science is an attempt to understand the universe as much as it is a technique to exploit nature; that—to use an expression of which he was fond—science is, above and beyond everything else, "natural philosophy."

Pasteur

As for Pasteur, it is often claimed that his scientific work originated from a concern with practical problems—for example, that his studies on fermentation had their basis in attempts to improve the quality of French wines and beer, or that his interest in infectious processes developed from efforts to save the production of silkworms in France. Nothing could be further from the truth. In reality, as I have shown elsewhere, Pasteur began his scientific life as a purely theoretical investigator, and he was already a famous scientist when he began to work on practical problems. From 1847 to 1857 his dominating scientific interests had no apparent practical significance; they dealt with the bearing of molecular structure on optical activity, and of stereoisomerism on the origin of life. A few years later he became engrossed in other abstract thoughts concerning the biochemical unity of life. As time went on, however, he yielded more and more to the social pressures of his environment and devoted the largest part of his productive life to practical problems of fermentation and disease. He became increasingly involved in using science as a technological instrument rather than as a discipline for understanding the universe. Repeatedly he expressed gratification at seeing that his labors would help man to gain mastery over the physical world and to improve human life. "To him who devotes his life to science," he wrote, "nothing can give more happiness than increasing the number of discoveries, but his cup of joy is full when the results of his studies immediately find practical applications."

There is no doubt, in my opinion, that Pasteur was aware that his involvement in practical problems interfered with the pursuit of his deeper scientific interests. He tried to justify his partial neglect of theoretical studies by the statement, "There are not

two sciences. There is only science and the applications of science and these two activities are linked as the fruit is to the tree." Yet, despite these brave words and regardless of immense success and popular acclaim, he must often have regretted the choice that had been imposed on him by the *Zeitgeist*. Time and time again he stated that he had been "enchained" by an inescapable forward-moving logic that had led him from the study of crystals to the problems of fermentation and contagious diseases. Yet, the desire of his early days to work on problems of deep philosophical significance apparently remained with him as a haunting dream. His grandson, L. Pasteur Valley-Radot, has recently told a moving story which reveals the pathetic intensity of this inner conflict during Pasteur's later years.

"He seemed to me serious and sad. He was probably sad because of all the things he had dreamed of but not realized. . . . I remember one evening, at the Pasteur Institute. He was writing quietly at his desk, his head bent on his right hand, in a familiar pose. I was at the corner of the table, not moving or speaking. . . . He stood up and, feeling the need to express his thoughts to the nearest person, even a child, he told me: *'Ah! my boy, I wish I had a life before me! With how much joy I should like to undertake again my studies on crystals!'* To have given up his research on crystals was the eternal sorrow of his life."

Conflict for Today's Scientists

Many modern scientists suffer from the schizophrenic attitude exemplified by Faraday and Pasteur. Fortunately, one particular aspect of science helps to minimize the inner conflicts generated by this attitude—namely, the fact, mentioned above and universally recognized, that it is often difficult to disassociate the theoretical from the practical aspects of science. Nevertheless, the conflicts are not entirely resolved by this interdependence of theory and practice. The uneasiness of scientists on this score is revealed by the observation that, whereas they claim among themselves that their primary interest is in the conceptual, not in the applied, aspects of science, in public they justify basic research by asserting that it always leads to "useful" results, meaning by this the development of processes and products that can be converted into social good, wealth, or power.

Like Faraday and Pasteur, scientists seem to find it somewhat embarrassing to admit in public that detached intellectual curiosity and a desire to understand the universe is one of the objects of science, whether or not "useful" results ever follow. Yet, despite Francis Bacon's claim that "knowledge, that tendeth but to satisfaction is but as a courtesan . . . ," the attempt to justify science only by its worldly products is fraught with dangers. Not only does it compromise the intellectual honesty of the scientific community, for reasons that need not be discussed here; in my opinion it also helps to foster among lay people some contempt for science itself.

The "Antiscience" Movement

Many are those in the course of history who have expressed doubts as to the ultimate value of natural sciences—from Socrates' skepticism to the talk of the bankruptcy of science so common around the turn of the 19th century. But these doubts did not necessarily mean hostility; rather, they expressed impatience at the fact that, despite oft-repeated promises, science had not solved the riddle of human nature and destiny. Far more dangerous, it seems to me, are the expressions of contempt for science as an intellectual discipline, and for scientists as thinkers, that have appeared repeatedly during the past few decades. Along with admiration and awe for the power of science there exists presently in the lay public, as pointed out by Margaret Mead, a curious mistrust of the scientist himself, as if he were something scarcely normal or human. This modern attitude toward the scientist is not far removed from that of primitive people toward the shaman or medicine man, whom they regard as an individual essential to the group but who is feared and often hated.

As typical of the hostile attitude toward science and scientists, I shall consider two books, published, respectively, in 1913 and 1930—namely, *The Tragic Sense of Life*, by Miguel Unamuno, and *The Revolt of the Masses*, by Ortega y Gasset. Both books have been translated into several languages and are still widely read and quoted today; they represent, and have spread far and wide, several aspects of the antiscience movement.

Unamuno and Ortega

Unamuno and Ortega recognize, of course, the contributions made by science to human safety and comfort. But while they appreciate the merits of aspirin and motor cars, they are very little impressed by the kind of intellectual process involved in the technology that has produced these conveniences. Most scientific thinking, according to them, corresponds to a performance of a rather low intellectual order. Just as ancient societies used slaves for the affairs of everyday life, so do modern societies produce and use scientific technicians for the same end. It is interesting to note here that, consciously or unconsciously, Unamuno and Ortega have accepted Bacon's claim that the scientific method is so mechanical and foolproof as to be readily and effectively handled by small minds.

As is well known, Bacon considered that induction, with absolute objectivity, was all that was needed to advance scientific knowledge and to convert it into social power. In fact, he had such confidence in this method that he thought it could be used, with success, almost blindly and by men of little talent. "My way of discovering sciences," he wrote, "goes far to level men's wits, and leaves but little to individual excellence; because it performs everything by the surest rules and demonstrations." Needless to say, few are the scientists today who believe that important discoveries ever result from the mere accumulation of facts. But by the general public the "scientific method" is still regarded as a more or less mechanical formula, different in quality from other creative processes. Indeed, Ortega and Unamuno seem to have taken to the letter Bacon's statement that "brutes by their natural instinct have produced many discoveries, whereas men by discussion and the conclusions of reason have given birth to few or none." As an extension of Bacon's aphorism, it seems worth while to quote here at some length from the several pages that Ortega devotes, in *The Revolt of the Masses*, to the low intellectual caliber of scientists and their discoveries.

"The actual scientific man is the prototype of the mass-man. Not by chance, not through the individual failings of each particular man of science, but because science itself . . . automatically converts him into mass-man, makes of him a primitive, a modern barbarian. . . .

"Experimental science has pro-

gressed thanks in great part to the work of men astoundingly mediocre, and even less than mediocre. . . . The reason of this lies in what is at the same time the great advantage and the gravest peril of the new science, and of the civilization directed and represented by it, namely, mechanization. A fair amount of the things that have to be done in physics or in biology is mechanical work of the mind which can be done by anyone, or almost anyone. . . . The work is done . . . as with a machine, and in order to obtain quite abundant results it is not even necessary to have rigorous notions of their meaning and foundations. . . .

"The specialist . . . is not learned, for he is formally ignorant of all that does not enter into his specialty; but neither is he ignorant, because he is a 'scientist,' and 'knows' very well his own tiny portion of the universe. We shall have to say that he is a learned ignoramus. . . . Anyone who wishes can observe the stupidity of thought, judgment, and action shown today in politics, art, religion, and the general problems of life and the world by the 'men of science.' "

Scientists having become so mechanical in their activities and so unconcerned with philosophical and truly intellectual problems, it is not surprising that, in Unamuno's words, "science does not satisfy the needs of our heart and our will." Not only does it not deal with the problems of the real man "of flesh and bone" but it "turns against those who refuse to submit to its orthodoxy the weapons of ridicule and contempt."

Thus, according to Unamuno and Ortega, the modern scientist is thoroughly dehumanized, does not see beyond his specialized techniques, and has no awareness of worth-while human goals. Science fails to deal with the problems that are the real concerns of mankind, and furthermore it stultifies all higher aspirations by fostering and satisfying the mass aspects of human nature. Lest there be an illusion that the antiscience movement is peculiar to Latin countries, I shall quote other remarks, originating from the Anglo-Saxon world. In Scotland, some 25 years ago, W. Mac Neile Dixon asserted in the Gifford lectures that "science is the view of life where everything human is excluded from the prospect. It is of intention inhuman, supposing, strange as it may seem, that the further we travel from ourselves the nearer we approach the truth, the further from our deepest sympathies, from all we care for, the nearer are we to reality, the stony heart of the scientific universe." This indictment should be supplemented with a related thought from an American author, "Science might almost be defined as the process of substituting unimportant questions which can be answered for important questions which cannot."

Sources of Misunderstanding

Scientists are inclined to find the reason for the antiscience movement in the fact that the public does not have the training or the ability required to appreciate the intellectual distinction and morality of scientific thought. But the possibility might be worth considering that the scientists themselves have a share of responsibility in this misunderstanding because they do not convey to the public the nobler aspects of the scientific heritage. It seems to me that some scientists have a tendency to exhibit pride of intellect in speaking of the "scientific method," as if it were something esoteric, superhuman in its power and precision, whereas in reality it is a very human activity supplemented by the use of specialized techniques. Instead of bragging about the purely professional aspects of a "scientific method" that we really cannot define, should we not emphasize more than we do the spiritual, creative, and esthetic aspects of all great scientific advances?

Scientists defend basic research in public by asserting that it cannot fail eventually to yield practical results, but they rarely advertise that knowledge per se is also a precious fruit of science. There is truth, of course, in Farrington's statement that "man makes his mental history in the process of conquering the world," but it is also true that science, like philosophy, has been pursued for its own sake, or rather, for the sake of intellectual satisfaction and of increased understanding. Long before there was such a thing as industrial technology, Ptolemy experienced the kind of intellectual intoxication that only knowledge can provide. "I know that I am mortal, a creature of a day; but when I search into the multitudinous revolving spirals of the stars, my feet no longer rest on the earth, but standing by Zeus himself, I take my fill of ambrosia, the food of the gods." In a similar mood Kepler also exclaimed, "Eighteen months ago the first dawn rose for me, three months ago the bright day, and a few days ago the full sun of a most wonderful vision." And at the end of his life Pasteur spoke lovingly of "the charm of our studies, the enchantment of science."

The Layman's View

The motivation which makes great scientists emphasize in public the practical worth of their studies and not their loftier intellectual goals is probably the wish to gain public approval. But there is no evidence that the public would not recognize and respect a purely intellectual motivation. In fact, it seems to me that whenever laymen have exhibited any interest in science they have been just as eager to learn of its philosophical aspects as of its practical applications. True enough, little is known of the manner in which the popularization of science was undertaken in past centuries and of the extent of its success; there does not seem to exist any thorough historical study of this interesting aspect of scientific communication. Nevertheless, there readily come to mind the names of many celebrated scientists who achieved great popular acclaim by bringing theoretical knowledge to lay audiences.

Public Response in the Past

Probably the best known, and certainly one of the first, of the science popularizers was Bernard de Fontenelle (1657–1757). He made his literary reputation with the *Entretiens sur la pluralité des mondes* and continued to hold the limelight with his more austere accounts of the achievements of scientists and of the "Académie des Sciences." According to his historian, L. M. Marsak, Fontenelle's writings had as many readers among the general public of the court and the bourgeoisie as among the learned; they went through six editions in his lifetime, and through six more by 1825. Fontenelle mentioned, of course, the practical potentialities of science, but what he emphasized was its humanistic quality, its contribution to enlightenment. He urged his readers to recognize that "Nature is never so admir-

able or so admired as when it is understood," and that it is at least as important for scientists to help the public rid itself of errors (*fausses merveilles*) as it is for them to proclaim true marvels. Would that all science reporters observed this admonition in our times! Although it would be out of place to write here at greater length of Fontenelle, I cannot forego quoting Marsak's appreciation of the magnitude of his achievement. "If the nineteenth and twentieth centuries have held up the businessman and engineer for emulation, it would not be an exaggeration to say that Fontenelle spoke for the civilization of the Enlightenment when he put the scientist in the niche that had formerly been reserved for the artistic creator of the Renaissance or the saint of the high Middle Ages." In addition to educating the public, Fontenelle helped create a friendly environment that eased the task of the scientists.

At the end of the 17th century and through the 18th century people came to Paris from all over Europe to attend lectures given by famous scientists. In London, the Royal Institution, with its lectures and demonstrations, long remained a fashionable rendezvous. In Germany, Helmholtz found it worth while to devote much time to presenting various aspects of theoretical science to the general public. Many other examples could be given to show that scientists have found, on many occasions, responsive audiences eager to learn not of processes and gadgets but of ideas and general laws. As evidence I need only quote a statement made by John Tyndall in Boston at the end of a highly successful tour during which he had lectured before lay audiences in the United States. "What, I may ask, is the origin of that kindness which drew me from my work in London to address you here, and which, if I permitted it, would send me home a millionaire? Not because I had taught you to make a single cent by science am I here tonight, but because I tried to the best of my ability to present science to the world as an intellectual good. . . . It is specially on this ground of its administering to the higher needs of the intellect; it is mainly because I believe it to be wholesome, not only as a source of knowledge but as a means of discipline, that I urge the claims of science upon your attention."

Public Response Today

It will be objected, perhaps, that times have changed, that the public is no longer interested in the large intellectual aspects of science but is concerned only with what technology can do for human comfort. Although this objection cannot be refuted convincingly, a few facts seem to be incompatible with it. For example, many of the books on science for the general public which became best sellers during recent decades dealt not with practical problems but, rather, with large theoretical themes of anthropology, biology, physics, astronomy—nay, even of mathematics—which could not in any way be practically useful in the conduct of the readers' material life. Here, again, an objective study of public response would be enlightening and could provide useful guidance for the popularization of science.

Earlier in this discussion I used the names of Miguel Unamuno and Ortega y Gasset to symbolize the movement which is often called antiscience. This was unfair to these authors because they are, in truth, the voice of humanity begging scientists to remember that man does not live by bread alone. They express, also, the fear of those who see science identified exclusively with power and technology at a time when it is beginning to reach populations which have never known it under any other guise. It should not be forgotten that in the Western world science was part of the culture for several centuries before it came to be used extensively for practical ends. Today this cultural heritage conditions, to a certain extent, the manner in which science is pursued and employed in the countries of Western civilization. In contrast, science is being introduced in the underdeveloped parts of the world not as a cultural pursuit but merely as a powerful and convenient tool—to be used at best for the production of material wealth, at worst for destructive purposes. It seems to me that scientists and science writers betray a public trust when they neglect to emphasize the disinterested aspects of knowledge and are satisfied instead, with claiming that all discoveries eventually prove of practical use. On the one hand, this is not necessarily true. On the other hand, this attitude ignores the fact that today, as in the past, men starve for understanding almost as much as for food. In the long run, the exclusive appeal to utilitarianism may well endanger the future of science and its very existence.

Understanding and Power

It is obvious, of course, that during recent decades science has improved the lot of man on earth even more successfully than Francis Bacon had anticipated. It is equally true, however, that for many centuries before the modern era, science had enriched mankind with a wealth of understanding at least as valuable as material riches. Scientists, like other men, win esteem and contribute to happiness more effectively through the exercise of wisdom than through the practice of power. And it is good for them to remember that, long before they had achieved technological mastery over nature and thus had become servants of society, their functions as high priests of natural philosophy had given them ancient titles of nobility which they must continue to honor.

Teaching and Research in Art Conservation

A university and a foundation join forces to help save our cultural heritage.

Rutherford J. Gettens

In a mansion on Fifth Avenue in New York City, home of the Institute of Fine Arts of New York University, the seed of a new idea has been planted, and it has taken root. The seedling is the new Conservation Center for art, which has been set up under a Rockefeller Foundation grant to train specialists who will be qualified to look after the physical welfare of cultural objects in museums and private collections.

Widely recognized as one of the most valuable segments of our cultural heritage is the art represented by paintings, drawings, sculpture, and other artifacts which has been bequeathed to us by past generations. Actually, only a small portion of the riches of the past has survived. Much has been destroyed by fire, earthquake, flood, war, and rebellion. A small residue and a precious one is left, but that is threatened by the most inexorable agency of destruction of them all—time itself, and slow decomposition. What disaster has spared, time may eventually destroy.

Fortunately, interest in art and its conservation is on the increase. Where formerly art collecting, exhibition, and preservation were left to private collectors, they are now being fostered by governments. The upsurge of nationalism the world over has made nations conscious of their past. In countries that formerly gave little heed to their cultural heritage there is now great pressure to establish museums, to promote archeological excavations, and to preserve ancient monuments. The present drive of the United Arab Republic to save the art treasures and monuments of ancient Egypt now threatened in Nubia by the proposed Aswan Dam Project on the upper Nile is a good example.

International Center

The problem of art conservation is so widespread and so urgent that UNESCO has taken cognizance of it to the extent that it has created in Rome an International Centre for the Study of the Preservation and Restoration of Cultural Property (*1*). Harold J. Plenderleith, chemist and former head of the British Museum Laboratory, has been made director. The purpose of the center is to survey the needs for art and monument protection all over the world; to collect technical information, to encourage education of conservation specialists, and to coordinate the research of museum laboratories. The Rome center is cooperating with the International Council of Museums, another UNESCO affiliate, in organizing and coordinating the work of museum conservation specialists. It is admitted that one of the greatest problems of the Rome center will be that of finding trained specialists who will actually do the work of preservation; finding specialists is perhaps the bottleneck the world over. There are countless problems. Apathy is slowly being overcome, money will eventually be forthcoming, but men with knowledge and know-how must be found. This is where the New York University Conservation Center can play a key role.

Early Practices

Formerly the treatment of works of art, especially of paintings in museums, was a casual matter. It was carried on at first by artisans from outside sources, later by staff employees. To quote William Boustead (*2*), conservator of the Art Gallery of New South Wales, these "were men of the old carver-gilder, picture-cleaner, craftsman type whose methods of restoration of pictures consisted of deep cleaning with 'spirit of wine' and turpentine, varnishing with copal resin and, worst of all, the pernicious habit of oiling out canvases with linseed oil. These tacky layers picked up dirt, dust, and mold spores, provided rich nourishment for mold growth and formed [over the picture] a tough leathery linoxyn skin which is now impossible to remove. Foxing of prints and drawings was removed with chloride of lime; drawings pasted down with flour paste and canvases lined with animal glue." Boustead paints an accurate picture. Such practices are no longer tolerated in the larger museums, but, unfortunately, they continue in some art dealers' and picture framers' private shops and among certain commercial restorers. This situation is clearly described by Sir Philip Hendy in the foreword to the catalog of an exhibition called "Cleaned Pictures," which was held in London in 1947 (*3*). He tells of the struggle that has been going on at the National Gallery, London, for over a century to preserve the 2000 paintings of that collection.

There were small beginnings towards a change in attitude in the 19th century. The first to recognize the need for a new approach to art conservation were officials of some of the larger European museums. The State Museum in Berlin established the first museum laboratory, in the late 1880's. The laboratory of the British Museum for Research in Conservation was installed in 1919, first under Alexander Scott; later it attained world-wide recognition under Harold J. Plenderleith. A little later the Museum of Antiquities at Cairo was set up under the chemist A. Lucas, whose name is still a by-word among Egyptologists. The establishment of a laboratory at the Louvre followed, in 1925. Another milestone in art conservation was reached about 1930 when the Department of Conservation was created at the Fogg Museum of Art, Harvard University. Modest scientific equipment was gathered at the Fogg Museum, and a definite program of research in the theory and practice of art conservation was initiated. The Fogg Museum became known especially for its pioneer publication, *Technical Stu-*

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dies in the Field of the Fine Arts, edited by George L. Stout, now director of the Isabella Stewart Gardner Museum in Boston. This journal did much to "increase and diffuse knowledge" in art conservation widely over the world. In the 1930's more museum technical laboratories were established: at the Museum of Fine Arts, Boston; the Metropolitan Museum of Art, New York; the Courtauld Institute, University of London; the National Gallery, London. Also in that decade the Belgians made museum laboratory and conservation history when they established, in Brussels, the Central Laboratory of the Belgian Museums (now l'Institut Royal du Patrimoine Artistique de Belgique), which has become world-famous under its energetic chemist-director, Paul Coremans. In the early 1940's the Istituto Centrale del Restauro was set up in Rome to give technical guidance for preservation of the rich patrimony of Italian art.

The author is shown examining vermilion-painted characters on a Chinese pottery urn of the type called *lien,* which was made about the 4th or 3rd century B.C. [Courtesy Freer Gallery of Art]

Government-Supported Programs

Most of these museum laboratories of foreign countries are government-supported and staffed by civil service employees. Some of them, including the British Museum and the National Gallery, advise, and to a limited extent render services to, provincial museums and even to private collectors. They accept an active responsibility for preserving the art treasures of the nations they serve (*4*). The ministries in Portugal, India, Japan, Canada, Poland, Yugoslavia, and some other countries are giving support to art conservation. In the United States, about the only government-supported art conservation is that carried on by the National Park Service. In taking over and operating historic sites, especially the historic houses of the East, the Park Service found it had assumed a considerable responsibility in the way of protecting art collections. The rich collection at Independence Hall, Philadelphia, of portraits of persons prominent in the founding of our Republic is an example. The Park Service had to set up a special conservation laboratory for care and treatment of these paintings. The Smithsonian Institution, also government-supported in part, is taking steps to establish a conservation department to look after its vast cultural holdings.

Another big event in art conservation was the incorporation under British Law, in 1950, of the International Institute for the Conservation of Historic and Artistic Works (called IIC). Its purpose has been to set professional standards in art conservation and to promote diffusion of knowledge of conservation methods, principally through its journals. The institute has two classes of membership: fellows, who are mostly professional conservators, museum laboratory administrators, and museum scientists; associates, who are interested laymen who want to keep in touch with the field. The institute publishes *Studies in Conservation* (edited at the National Gallery, London), *IIC Abstracts of the Technical Literature in Art and Archaeology* (edited at the Freer Gallery of Art, Washington, D.C.), and *IIC News* (London). The institute, which now has over 600 members in 49 countries, gets most of its support from members and private institutions and a little from foundations (*5*).

Work of Private Institutions

A few institutions in this country, using private funds, are also carrying the torch for conservation and technical studies in art. In the early 1950's the National Gallery in Washington, through grants from the Old Dominion Foundation, set up a fellowship for research in artists' painting materials at the Mellon Institute in Pittsburgh. Intensive research on protective coatings for pictures is carried on there by Robert Feller. As a result, the natural resin varnishes formerly used for coating pictures, which cracked and yellowed with time, are slowly being discarded for synthetic polymer coatings, which do not yellow and do not deteriorate so rapidly. At the Intermuseum Laboratory in Oberlin, Ohio, which is supported by several museums of the Midwest, procedures for the systematic examination and maintenance of whole art collections are being devised (*6*). In 1951 the Freer Gallery of Art inaugurated its Laboratory for Technical Studies in Oriental Art and Archaeology. This laboratory is mainly equipped and operated as a chemical analytical laboratory where studies on the materials and construction of ancient Chinese bronzes, ceramics, paintings, and other artifacts are made (*7*). Similar activities are going on in the Walters Art Gallery in Baltimore, the Isabella Stewart Gardner Museum in Boston, the University of Pennsylvania Museum, and elsewhere.

What the art museums in this country do is, at best, largely governed by practical demands in conservation, and the same is true, by and large, abroad. They are occupied with the care of their own collections, and even this activity is mostly limited to emergencies as they arise. They do not find themselves in a position to undertake investigations that are broad in scope, or to

maintain courses of academic instruction in the field. Hence, at best, their contribution to the advancement of knowledge and the raising of standards in art conservation is slight. At worst, their chief interest, like that of art dealers, is in restoration rather than conservation—in obscuring damage and making objects look whole and attractive for exhibition, not in preserving them for the future.

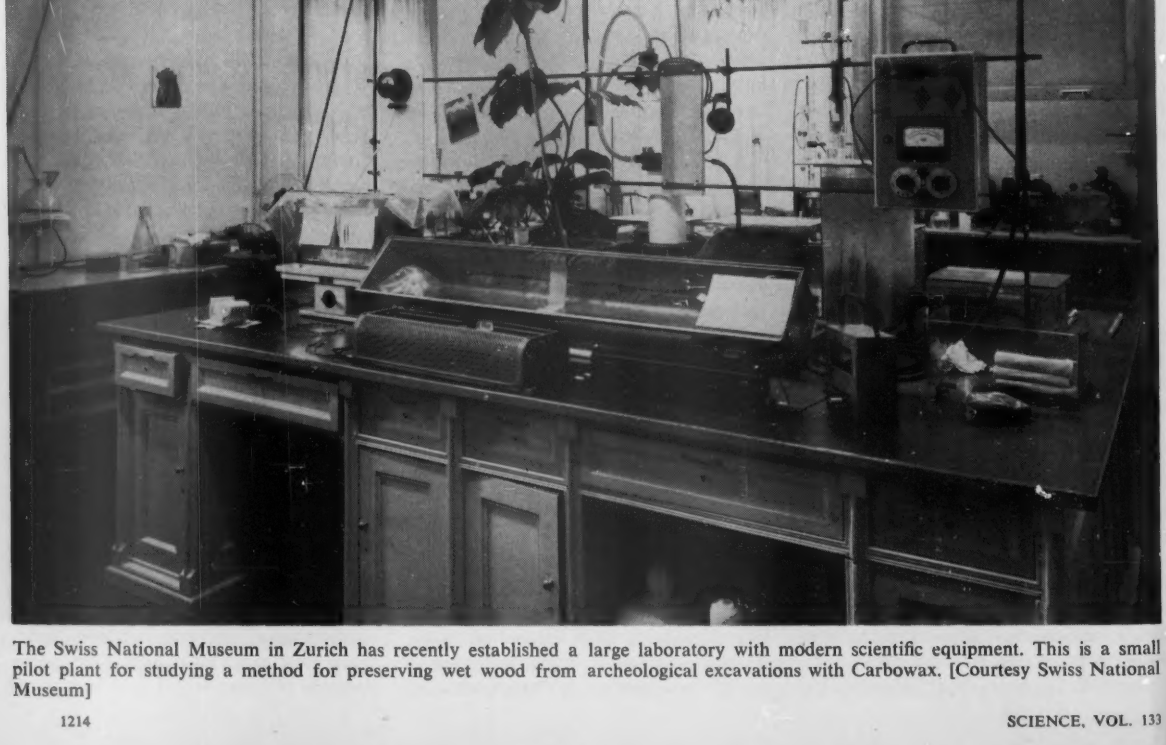
Role of the Private Foundation

The private foundations are beginning to be aware of the need to conserve our cultural heritage. In 1957 the Eli Lilly Endowment sponsored a conference on "Protective Coatings" at Oberlin College, Ohio, under the guidance of Richard D. Buck. This meeting was attended by some 50 curators and conservators, and it eventually led, in 1959, to the publication by the Intermuseum Conservation Association of a 220-page report, *On Picture Varnishes and Their Solvents*. In 1959, also, the Rockefeller Foundation sponsored a conference (and publication) on "Application of Science in the Examination of Works of Art," held at the Museum of Fine Arts, Boston, under the chairmanship of William J. Young, director of the Research Laboratory of the museum. That same year the Rockefeller Foundation also gave support for an "Exploratory Conference on Art Conservation" at the Brooklyn Museum. This conference, organized by the late Edgar C. Schenck and by Caroline and Sheldon Keck, director and conservators, respectively, of the museum, was attended by museum directors, curators, scientists, and foundation representatives, and the discussions paved the way for the grant later made by the Rockefeller Foundation for the support of the Conservation Center at New York University.

It may be seen from this account that space, equipment, and money are being made available to rescue our vanishing cultural heritage. The main problem is to provide trained individuals to use the tools that are being provided. Most of the conservators and administrators of museum laboratories are self-trained. Some got their training under a sort of apprentice system. Several are scientists with advanced degrees. There are many dedicated and capable people in the field. The time is rapidly approaching when professional training in conservation must be made available. The field is becoming so important and the problems are so vast that universities must provide men with the professional training and stature that the job requires. Unfortunately, except to some degree at the Courtauld Institute, University of London, research and practice in museum conservation are nowhere associated with regular university courses in the fine arts.

Conservation Center

To fill this need for trained manpower is what the new Conservation Center at New York University aims to do. The Rockefeller Foundation grant will support the center entirely for five years, then the grant will taper off, and after another five years the center will have to find other means of support. Nearly the entire basement (formerly the kitchen and service areas) of the James B. Duke house at



The Swiss National Museum in Zurich has recently established a large laboratory with modern scientific equipment. This is a small pilot plant for studying a method for preserving wet wood from archeological excavations with Carbowax. [Courtesy Swiss National Museum]

Stephen Rees Jones, lecturer-in-charge at the laboratories of the Courtauld Institute of Art in London, is shown with students removing the paper facing from a painting which has been reinforced with a new canvas by means of the original relining equipment developed in the institute laboratories. [Courtesy Courtauld Institute of Art]

1 East 78th Street, home of the New York University Institute of Fine Arts, has been given over to the Conservation Center. During the past summer this basement was extensively remodeled to provide a large student workroom, a chemical laboratory, an x-ray laboratory, a workshop, and three offices. Scientific instruments and equipment include a modern x-ray machine, a 3-meter Jarrel-Ash grating spectrograph, various microscopes, a chemical balance, and chemical glassware. There are also a source of ultraviolet light, a sodium lamp, and infrared photographic equipment. The equipment will be used by the staff in conducting research dealing with materials and methods of art conservation. It will also be used by the more advanced students. In the beginning, problems of an *ad hoc* nature will be worked on. These will include uses of iron-55 isotopes in examining low-density materials such as paper and tissues; use of polyacrylonitriles as adhesives and surface coatings for paintings; and spectrographic identification of trace elements in ancient Chinese bronzes. Later, problems of long-range importance, such as that of preserving Oriental paintings on silk and paper, will be tackled. This is an especially urgent problem because there are thousands of irreplaceable Chinese and Japanese paintings in American collections and no one now has the proper knowledge and skills to prevent them from deteriorating and ultimately disintegrating.

In setting up the Conservation Center in the fall of 1960, the officials of New York University ran into the very problem they are trying to solve for other institutions—that of procuring a top-flight staff for research and instruction. The program calls for a resident teaching staff in both conservation and science. Lawrence J. Majewski, experienced in the conservation of both easel paintings and wall paintings, is the conservator, with the title of research associate, lecturer, and administrator. George L. Stout, mentioned above, commutes periodically from Boston to New York to aid in the work of the center. A board of consulting fellows, consisting of Stout (chairman), Murray Pease (head of the Conservation Department of the Metropolitan Museum), Edward V. Sayre (chemist, Brookhaven National Laboratory), and me, sets policies, establishes programs, and evaluates the students' work. Sayre will have the position of Scientist for the first half of the first year, and Robert L. Feller of the Mellon Institute, for the second half. The appointment of a permanent Scientist has not yet been made.

Training

In the fall of 1960, five graduate students were accepted for training. Prerequisites were some undergraduate courses in design, courses in drawing and painting, and some acquaintance with the physical sciences (the equivalent of at least 12 credit hours). Under the program, as set up, the students were enrolled in three history of art courses in the university and, as part of their training in conservation, they were enrolled also in a new course called Materials and Examination of Art. This course provides systematic instruction in the properties and classes of art materials, including stone, ceramics, metals, paints, pigments, and organic support materials such as wood, fabric, and paper. It also provides for systematic study and reporting on objects made from these materials. In the second year, after they have completed more courses in art history and in materials of art, the students can qualify for an M.A. degree in art history. In the third and fourth years the students who wish to become professional conservators will take special courses in museology and connoisseurship, arranged through the cooperation of the Metropolitan Museum. They will be given, also, advanced instruction in the materials and construction of art objects, in environmental influences, and in the practice of conservation. At the end of this period students can qualify for a diploma in Conservation of Art.

The purpose of the training program

of the Conservation Center is to turn out not only professional conservators but also a new type of curator with a fundamental knowledge of materials and structures of works of art, with some experience in applying conservation techniques, and with a thorough grounding in history of art—all on the graduate level. Curators with this special background will eventually fill positions of responsibility in our larger museums, where the care and maintenance of collections is as important as their interpretation. Those who complete the four years of training and qualify for a diploma in Art Conservation will be in demand to head museum conservation laboratories, which are becoming more numerous and more important all over the country. Others may find private work in conservation more interesting and lucrative. Private conservators will have the task of caring for the vast cultural holdings still in private hands. Their work will be as important as that of workers in museum laboratories. Like university graduates in the established professions such as medicine, engineering, and the military services, graduates in art conservation may find that their specialized training qualifies them for varied careers. There is little doubt that the standards set by this university-level training program in art conservation will raise conservation standards all over the country. We may look forward to the establishment, on the basis of such foundations, of minimum professional requirements and even to state licensing for art conservators. Men trained in the Conservation Center of New York University will have an influence with museum administrators that artisans could never have achieved. They will help frame policies and introduce practices that will extend the life of cultural objects many hundreds of years.

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3. P. Hendy, in "An Exhibition of Cleaned Pictures (1936–47)" (National Gallery, London, 1947).
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Sarsen Stones of Stonehenge

How and by what route were the stones transported? What is the significance of their markings?

Patrick Arthur Hill

The concentric stone circles of Stonehenge, England, consist of two main types of stone, bluestone and sarsen. Both have been transported many miles from their original outcrop areas, the bluestones from the Prescelly Hills 140 miles to the west (*1*), the sarsens from the chalk Downs 21 miles to the north. To date, the bluestones, because of the greater distance transported, have received the greater attention, although the sarsens are larger and several times heavier.

Sarsens, in their natural state, consist of large blocks of silicified sandstone that lie at certain localities on the Downs. The nearest outcrops to Stonehenge are shown in Fig. 1, and there is no evidence to suggest that the stones—some weighing as much as 50 tons—were moved to Stonehenge by agencies other than human. The problem is, how and along what route were the stones transported? At the time of movement, in the early Bronze age (about 1500 B.C.), there were supposedly no wheels in Britain, no beasts of burden, and, other than rawhide, no rope (*2*).

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Route

In the only study of the transportation problem to date, Atkinson (*3*) suggested that the Stonehenge sarsens originated on the Marlborough Downs, because the largest outcrops occur there today. According to him, selected sarsens were dragged to Avebury, "blessed," then dragged one at a time on sledges over hardwood rollers to Stonehenge. To avoid the steep descent of the chalk escarpment, Atkinson swings his route westward to cross the Vale of Pewsey at its narrowest point (see Fig. 1) and, using this route (*3*, p. 115), calculates that the 81 sarsen stones were moved one at a time at an average speed of a half mile a day and that the task occupied 1500 men for 10 years. Later (*4*) he suggested that the *minimum* hauling party for the heaviest stone would have been 100 men most of the way and 450 up Redhorn Hill. These figures are for the haulers only and do not include the additional men required to shift the rollers and steer the sledge.

Despite the advantages, there are six objections to Atkinson's route. (i) The labor requirements are large. (ii) The route is 4 miles longer than necessary. (iii) A surprising number of sarsens outcrop south of the Kennet; many admittedly are smaller than those on the Marlborough Downs, but this may simply mean that the larger stones have been removed. (iv) Avebury does not lie on the direct route to Stonehenge. (v) It would be easier to have the stones blessed *in situ*. (vi) The ascent of Redhorn Hill is unnatural and unnecessary, and more than negates any advantage of the detour.

A possible alternative route (*5*), based on field work undertaken during 1958, is suggested in Fig. 1. It has the following advantages. It is shorter than Atkinson's route and is essentially downhill for 17 of its 21 miles. It would account for the pebbles of opalescent quartz found in some of the Stonehenge sarsens, because opalescent quartzes occur in sarsen outcrops at Lockeridge (*6*), which is on the route. It also connects with the Stonehenge avenue at Amesbury, and there is thus

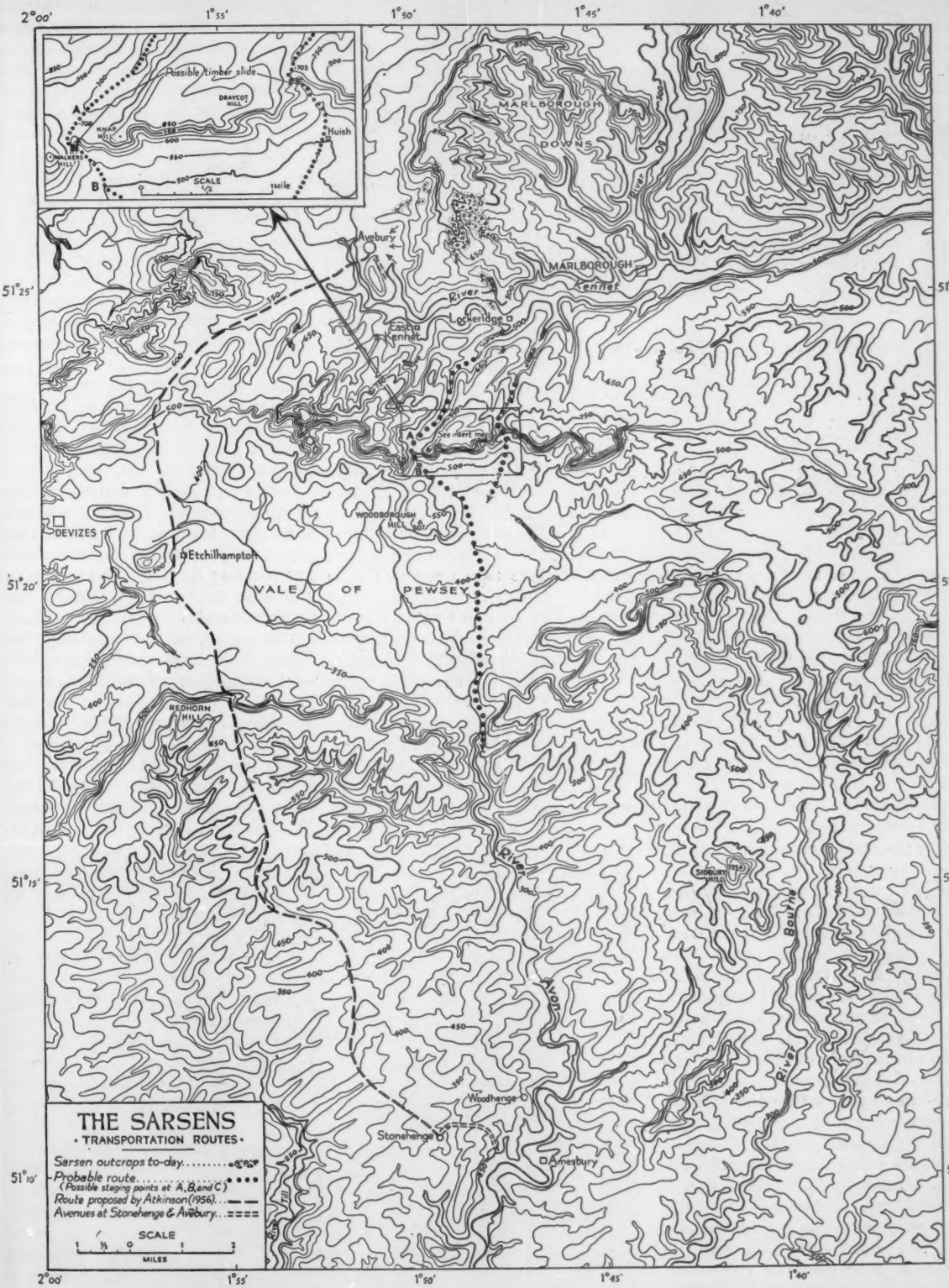

Fig. 1. Possible transportation routes to Stonehenge. [Contours from 1 in./mi. Ordinance Survey Sheets]

Fig. 2. The Avon at Upper Woodford, 4 miles south of Amesbury, looking south. (Width of river, 8 to 15 feet; average depth, 2 feet.)

no need to explain how the sarsens were dragged across the avenue.

This alternative route may be divided into five stages: (i) a steady but gentle climb for 3 miles from Lockeridge (500 feet) to the saddle (708 feet) between Walkers Hill and Knap Hill, with a possible subsidiary route at Huish; (ii) a slide or slides down the chalk escarpment; (iii) the crossing of the Vale of Pewsey; (iv) the journey down the Avon Valley; (v) the journey up the avenue from Amesbury. For the moment it may be assumed that the stones, except in stage (ii), were hauled in the "conventional" manner, on sledges over wooden rollers.

The free slide assumed for stage (ii) would have been the easiest, fastest, and safest way of lowering the sarsens the necessary 150 feet. The location of the slides is conjectural, but the locations shown are thought to be the most likely. Slides at these locations would have been the steepest and the shortest, and the nearest to the gaps through the Downs. The areas are sheltered from the westerly winds and are also partly sheltered from the sun. The slides may have been over snow, grass, or bare chalk. The sarsens may have been slid downhill on their sledges or tipped off and slid down on their own, to accumulate at the bottom to await the next stage of their journey. If this sliding-without-sledges was the method used, the stones might have been slid broadside over debarked tree trunks laid on (and pointing down) the hillside and fitted together end to end.

Along the Avon valley [stage (iv)] the route lies east of the river, to avoid the two tributaries. The stones, if the depth of water were the same as today (2 feet), could not have been floated; they must have been dragged, and in places, to avoid river bluffs, they may have been dragged through the water for some little distance (with the incidental gain of buoyancy). The number of river crossings is unknown: meanders may have changed position. To judge from the present position of meanders, however, only one or two crossings would have been necessary.

The idea that the stones might have been accumulated at staging points is logistically attractive. Each stage of the route presents its own problems, and each, for maximum efficiency, might have been the responsibility of one group of workers. Eating and sleeping arrangements would have been simplified, and the distance that each group would have had to travel would have been decreased.

Climate

So far it has been assumed that the climate and vegetation in 1500 B.C. were essentially the same as today. Climates, however, have changed repeatedly in the past and are changing today.

The changes since the last maximum of the Würm or Wisconsin glaciation are outlined by Brooks (*7*); according to him, "the sub-boreal climate (2500–500 B.C.) was peculiar. On the whole there was a gradual decrease of temperature and rainfall from the Climate Optimum [(*8*)], but this was interrupted by long droughts in which the surface of the peat dried up, followed by returns to more rainy conditions. This alternation occurred several times, the main dry periods falling about 2200–1900, 1200–1000, and 700–500 B.C."

Unfortunately, much of the evidence for climatic change "beyond the reach of the very short instrumental record is vague, and most of it is purely qualitative" (*9*). As for the local scene, Kennard and Woodward (*10*), from findings of nonmarine mollusks, concluded

Fig. 3. The heelstone, Stonehenge looking north. *A-B* older bollard marks; *D*, younger bollard marks; arrows spalling. (Scale: the book is 9 inches high.)

that the rainfall in Wiltshire was "much heavier," the downs "more wooded" (with beech), and the water table "much higher" than at present. On the other hand, Atkinson (*4*) thinks that the local climate was "distinctly drier," because wind-blown silt is found "not merely in the Y and Z holes of Stonehenge itself, but also in the ditches of broadly contemporary round barrows." Dimbleby (*11*), after preliminary pollen sampling from Windmill Hill, 1 mile northwest of Avebury, concluded that "the downs were more wooded, mainly with hazel" but hesitated "to translate such observations into terms of climate."

Willett (*12*), from data on sunspots for A.D. 1750 to 1950, derived a cycle of 80 to 90 years "consisting of alternating phases, each 40–50 years long, of maximum and minimum sunspot activity corresponding to nonglacial and glacial conditions respectively." Colder periods of longer duration are known to have existed before 1750, but it would be unwise to extrapolate sunspot information back to 1500 B.C. (*13*). On the other hand it is reasonable to assume that climatic fluctuations—duration and cause unknown—existed then as now. Accordingly, cautious speculations may be made about the effect of (i) a wetter climate and (ii) a colder climate on the transportation problem.

A wetter climate implies that the Avon was deeper and wider, and that the sarsens could have been floated downstream. Today, the average depth and width of the river are 2 and 10 feet, respectively (Fig. 2). A medium-sized sarsen block, 25 by 4 by 2½ feet, lying flat-side-down on a 25- by 12-foot wooden raft, would require a minimum depth of 5.6 feet (*14*). Whether the river was ever as deep as this would be difficult to determine; the whole river valley is artificially channeled.

The possibility that the Avon was dammed at, or below, Amesbury to obtain the required head of water has to be considered. Technically, the sarsen movers were quite capable of constructing a timber or earth dam; Silbury Hill (*15*), an earth mound 130 feet high, is a monument to their industry and skill. The evidence for such a dam, however, has yet to be found, and moreover, unless the regional water table were markedly higher than it is now, it would have been difficult to maintain the required level, for chalk makes a poor reservoir rock.

A colder climate does not imply that conditions were polar or subpolar—merely that winters were slightly colder than at present. In fact, as shown by Niven (*16*), subzero temperatures would have worked against the sarsen movers.

On ice, the sarsens could have been moved by a comparatively small number of men using sledges. One man properly shod can move a 2-ton Eskimo *komatik* (*17*), so 25 men could move the largest, 50-ton sarsen. Niven (*18*) believes that, once the sledge had been started, the number of men could have been reduced to 15, especially if, as believed, the sledge were moving down a gentle slope. The river, however, would have had to be frozen solid—a condition which presupposes severe continental winters.

Under less extreme climatic conditions there would have been packed snow, not ice. On average packed snow, one man can move a 1-ton *komatik*, and thus 50 men could move the heaviest sarsen. Unfortunately, "average" snow is difficult to define.

Snow deposits may be classified into five groups (*19*), of which, if the climate of prehistoric Britain did not differ too much from today's, the wet,

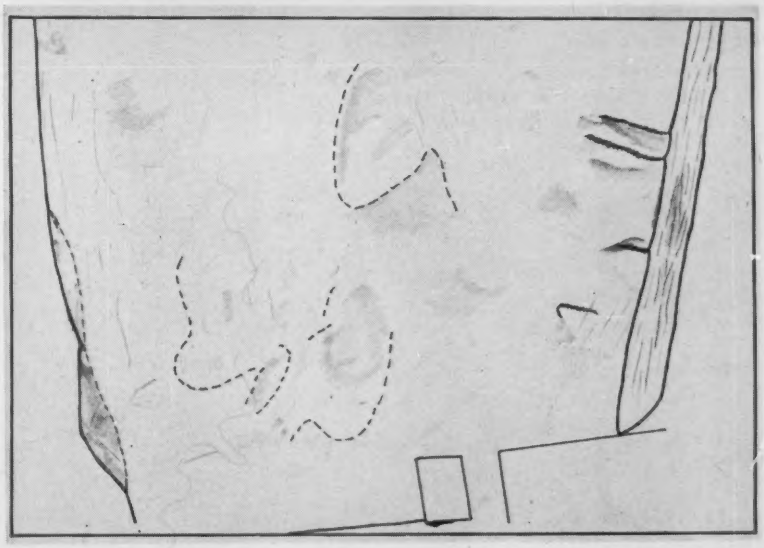

Fig. 4 (top). The lower half of the northeastern (inner) face of sarsen 16, Stonehenge. Three of the five grooves are indicated. The three crouching "figures" are in the center and at lower left (the figures are difficult to photograph; they show more clearly in color). (Scale: the book is 9 inches high.) Fig. 5 (bottom). A drawing made from Fig. 4 to show figures and tooling marks.

Fig. 6. The northwestern edge of sarsen 16—a close-up of the three grooves shown in Fig. 4. [Scale: the coin (at top, center) is 1.2 inches in diameter.]

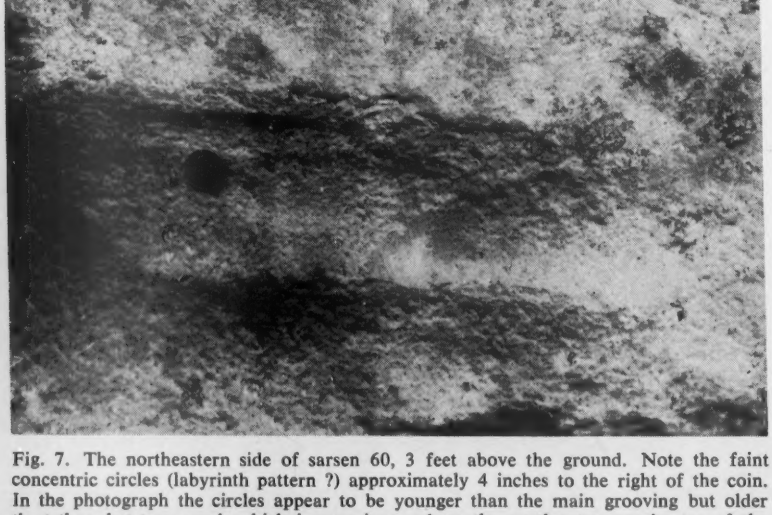

Fig. 7. The northeastern side of sarsen 60, 3 feet above the ground. Note the faint concentric circles (labyrinth pattern ?) approximately 4 inches to the right of the coin. In the photograph the circles appear to be younger than the main grooving but older than the whetstone mark which is superimposed on the southeastern perimeter of the circles. [Scale: the coin (left of center) is 1.2 inches in diameter.]

soft, fluffy snow of group 4 and the slush of group 5 would have been most common. When fresh, snow of the group-4 type offers considerable resistance to forward motion, although when packed its resistance decreases. Slush offers minimum resistance, "provided there is a solid foundation of snow or ice" (*19*). The best combination of snow and ice for heavy-duty transportation would be "ice roads" made from packed snow by the repeated passage of sledges or by the spraying or spilling of water. Although such roads have been used in northern Europe and North America for centuries, two interesting and related facts in this connection are not generally known.

First, insofar as the kinetic friction of ice is concerned, Amonton's law (*20*) does not always hold true. After a certain load is put on a sledge, "the drag increases but little by additional amounts" (*21*). Second, the best temperatures for moving very heavy loads are those just below 29°F, temperatures common in Britain in winter. Subzero temperatures on the other hand would increase frictional drag very markedly (*22*). Furthermore, to increase the longitudinal flexibility of the sledges and to improve handling and maneuverability, the stones were probably moved [except in stage (ii)] with their curved side downward and their flat side uppermost. This would also make it easier for the movers to re-start a frozen-in sledge.

The sarsens may have been moved along part of the route at night, particularly in the spring, when the best ice conditions obtained. With the Lockeridge-Avon Valley route and a prepared surface, each stone could have been moved in stages to Stonehenge at a slow walking pace, and its total time in transit could well have been less than a week. Green (*23*) believes that "If the blocks were dragged over grass during the summer months there would be the difficulty of providing labor at the time that labor was needed for agriculture. If however the blocks were transported during the winter, labor would be more readily available and the transportation of the stones would provide a social event to relieve the relative inactivity of the winter months."

If Newall (*24*) is correct in suggesting that the Stonehenge sarsens were laid out as they were to observe not the midsummer sunrise but the midwinter sunset, the main ceremonies would have been at dusk, and they might have been synchronized with those at Avebury by signal fires. The in-

tersection in Sidbury Hill of the Stonehenge axis and the approximate axis of the Avebury avenue might not, therefore be coincidence (*25*).

To sum up, the suggestion that the accumulation of snow (or the amount of frost) was greater at the time the sarsens were moved cannot be proved, but it cannot be disproved. Since glacial times the climate has gradually become warmer, and as many as 13 minor oscillations of moisture and temperature are reported (*26*). In fact it might, even today, be possible to move the sarsens by sled to Stonehenge from one point to the next during selected winter periods.

Markings on the Stones

Although Stonehenge, the "most celebrated monument in Europe," has been the subject of numerous monographs, prehistoric carvings were discovered on the sarsens only in 1953 (*27*). Of the carvings the most important is a "Mycenaean" dagger—a finding which suggests that a Mycenaean architect may have supervised the erection of the stones. Of other man-made marks the only ones described before 1953 were tooling and dressing marks (*28*). Even today no marks indicative of quarrying, transportation, or erection are mentioned in the literature.

Current work has disclosed some undescribed markings on the heelstone and on sarsens 16, 28, and 60.

The heelstone is set approximately in the center of the avenue, outside the main sarsen structure. It rises 16 feet above the ground and is sunk an additional 4 feet in the ground. The stone leans noticeably toward the center of the avenue but, presumably, originally was upright. The naturally smooth east face has been polished and grooved. There are markings which appear to be bollard marks of two ages (Fig. 3). The earlier (*A-B*) is at right angles to the axis of the stone and was apparently made before the stone heeled over (*29*). It indicates that an object of some kind was dragged along the ground away from Stonehenge. The later bollard mark is higher and opens out at both ends.

As unpolished stone is highly abrasive, it is doubtful whether the polishing was done originally by rope. The stone might have been polished in preparation for use as a bollard. The polishing may not be prehistoric, but on the other hand sarsen 55, diametrically opposite the heelstone, has a bulbous protecting foot (*28*) above which material has been removed and the stone has been planed.

The most interesting marks are on sarsen 16, a wedge-shaped stone which like many Stonehenge sarsens has a curved outer face and a flat inner one. The marks are all on the inner face; there are three crouching figures and a series of grooves (Figs. 4 and 5). The figures are superimposed on the pocked "orange-peel" finish (*30*) of the stone and appear to be younger than the grooves on which the finish is superimposed.

The highest figure is the most prominent. It seems in part natural—a freak identified as "human," and accentuated —and may have served as inspiration for the other two figures (one steatopygous), which appear to be wholly man-made. These two figures, unlike the other carvings of Stonehenge, are not recessed but have been ground into the rock surface with a stone maul or an axe blade. The only other explanation for the figures is that sarsen 16, at one point on its journey to Stonehenge, was levered and swiveled side to side, flat face downwards, over hard round cobbles. It would, however, be difficult, if not impossible, to produce "figures" by this method.

The grooves on sarsen 16 are five in number and occur only along the one edge. Three of them are parallel. The grooves may date from historic times and may have been formed *after* the erection of the stones—that is, sarsen 16 may have been used as a whetstone for sharpening iron or steel implements.

Fig. 8. The northwestern edge of sarsen 60. The prominent grooves underneath the orange-peel finish may be dressing marks. However, their position on the edge of the stone is unusual (?) for dressing marks, and other interpretations are possible. [Scale: the coin (center) is 1.2 inches in diameter.]

However, the upper two grooves are too high for this purpose; the orange-peel finish, assumed to date from the time of erection, is superimposed on the grooves, and the grooves are confined to one edge. On the other hand, if the grooves were made by the builders of Stonehenge, several interesting interpretations are open to consideration. These, in order of increasing probability (*31*), are as follows. (i) Sighting marks—unlikely (*p*, less than 0.1). The grooves are too wide. They aim upwards into the sky, and even if all sarsens were re-erected, the grooves would sight open sky. (ii) Counterweight marks (that is, marks resulting from use of the stone as a moving counterweight in, or on, a crude balance or derrick)—unlikely (*p*, less than 0.1). The stone is not rectangular. It is wedge-shaped. Its center of gravity is so off-center that any engineering use is improbable. (iii) Dressing marks—unlikely (*p*, less than 0.2). The grooves are confined to one edge and do not resemble the tooling or dressing marks hitherto described. (iv) Rope scars made in erecting or demolishing the sarsen structure—unlikely (*p*, less than 0.2). The marks are underneath the orange-peel finish. They are too wide and too deep, and their longitudinal axes are not sufficiently convex. Moreover, the outer lips of the grooves are sharp, whereas if made by ropes they would be rounded. (v) Axe marks —possible (*p*, less than 0.5). However the grooves do not resemble other axe-sharpening marks; they are too long and too deep. Axe marks where only the blade has been sharpened are flat (*3*, plate 24a). Where the axe face has been repeatedly resurfaced, a groove does develop, but its longitudinal axis would appear to be concave upwards (*32*). (vi) Wedge marks—possible (*p*, near 0.5). But normally only iron wedges could cause such prominent marks, and usually wedges are driven in perpendicular to the working surface, not at an angle. If, however, the wedges were driven in by alternately lifting and dropping a heavy weight—that is, by gravity instead of by hammering—the grooves would be parallel because they would all originally have been vertical. In this case, the working surface must have been inclined. If wooden wedges were used, the grooves could be made by first filing a hole with a flint sand, water, and wood stakes and then increasing the diameter of the stakes until the hole was ready to take the final wedge (all of which seems unnecessary and laborious).

Finally (vii) we have the most controversial interpretation—that the grooves may represent slide or friction marks caused by the stone's sliding downhill over wooden logs. This is perhaps possible (*p*, ?) (*33*). The grooves, especially the longest, "fit" logs (Fig. 6). Their longitudinal axes are slightly convex, suggesting that the slide and the hillside on which it rested were correspondingly concave. If this interpretation is correct the angle between the grooves and the edge of the stone would indicate that the stone did not move downhill broadside on, but that the thicker, heavier end led slightly.

Although there are grooves on other sarsens, few appear to have been caused by sliding. Most appear to be dressing marks. Marks indicative of quarrying and transportation, if originally present, have apparently been obliterated. This is perhaps to be expected, because the flat inner faces, the ones which might be expected to show friction grooves, have been most carefully dressed. In addition, many stones are deeply weathered, and the edges of many others are chipped and destroyed.

Nevertheless, grooves are present on sarsens 60 and 28 (Figs. 7 and 8). Those on sarsen 60 were subsequently used for sharpening axes. The groove on sarsen 28 is high above the ground on the northwestern edge. It is prominent and horizontal, approximately 6 inches wide and 1 foot long, and appears to have a slightly convex longitudinal axis (?). Below it are three man-made grooves with concave longitudinal axes, made apparently to remove a protuberance.

Sarsen 16, with its prominent grooves and figures, might have been of special significance and might have been the first stone to have been slid downhill.

Summary

A route via Lockeridge and the Avon Valley, involving a slide down the chalk escarpment, is postulated for the sarsen stones of Stonehenge. The transportation problem would have been greatly simplified if the stones had been relayed from point to point over snow or slush during successive winters. Markings on the stones hitherto undescribed are interpreted.

References and Notes

1. H. H. Thomas, *Antiquaries J.* **3**, 239 (1923).
2. This supposition is based on negative evidence. Conceivably oxen might have been available for draft purposes, and rope other than rawhide might have been in use. See N. Lockyer, *Stonehenge and Other British Stone Monuments Astronomically Considered* (Macmillan, London, 1909), p. 84; G. W. B. Huntingford, *Antiquity* **8**, 456 (1934); J. G. D. Clark, *Prehistoric Europe, the Economic Basis* (Philosophical Library, New York, 1952), pp. 101, 122, 226.
3. R. J. C. Atkinson, *Stonehenge* (Hamilton, London, 1956).
4. ———, written communication.
5. The interest of R. J. C. Atkinson is gratefully acknowledged.
6. R. J. C. Atkinson, oral communication.
7. C. E. P. Brooks, in Malone, *Compendium of Meteorology* (Am. Meteorol. Soc., Boston, Mass., 1951).
8. W. C. Cooper [*Bull. Geol. Soc. Am.* **69**, 941 (1958)] rightly objects to this misleading expression.
9. R. F. Flint, *Glacial and Pleistocene Geology* (Wiley, New York, 1957).
10. A. S. Kennard, *Wiltshire Archeol. Nat. Hist. Mag.* **45**, 235 (1932); ——— and B. B. Woodward, *ibid.* **45**, 334 (1932).
11. G. Dimbleby (University of Oxford), written communication.
12. H. C. Willett, *J. Meteorol.*, **8**, 1 (1951).
13. Such an extrapolation would be unwise "especially as the problem of solar influences on weather is confused; some correlations seem to show in phase and others anti-phase relationships," to quote from a letter from J. W. Locke, Dominion Observatory, Canada.
14. For this calculation a specific gravity of 2.4 for sarsen and of 0.60 for wood, and a 10-percent volume loss (void space) for the raft were assumed.
15. Silbury Hill, 1 mile south of Avebury, is not to be confused with Sidbury Hill.
16. C. D. Niven, *Eng. J. (Montreal)* **39**, 226, 227, 230 (1956).
17. Information supplied by R. G. H. Williamson and P. Murdoch of the Arctic Division, Ottawa, and by W. E. Taylor of the National Museum, Ottawa, is gratefully acknowledged.
18. C. D. Niven (National Research Council, Ottawa), verbal communication.
19. W. E. Wakefield, *Publ. Forest Prods. Labs., Can., Div. Timber Mech., Project 107* (1938).
20. Amonton's law states that the frictional force acting between two bodies is proportional to the weight of the top body (or total pressure acting normal to the boundary).
21. C. D. Niven [*Eng. J. (Montreal)* **39**, 226 (1956)], quoting W. E. Wakefield.
22. ———, *ibid.* **39**, 226 (1956).
23. R. Green (University of Tasmania), written communication.
24. R. S. Newall, *Antiquity* **3**, 75 (1929).
25. I introduce this idea with caution. Intersections have been unpopular ever since Lockyer [*Stonehenge and Other British Stone Monuments Astronomically Considered* (Macmillan, London, 1909)] used clock stars, May years, and intersections to "date" barrows, stone circles, and avenues. The Avebury avenue is, moreover, slightly sinous and of two different periods of construction.
26. L. von Post, *New Phytologist* **45**, 192 (1946); J. Gordon Ogden, *Am. J. Sci.* **258**, 341 (1960).
27. As most published photographs are not scaled, this oversight is, perhaps, not surprising.
28. E. H. Stone, *The Stones of Stonehenge* (Scott, London, 1924).
29. Alternatively, could the heelstone have stood in another position? Compare the outline of socket *E* with that of the heelstone in Atkinson (*3*).
30. This pock-marking or pitting is believed by both Stone (*28*) and Atkinson (*3*) to represent the final dressing of the sarsens. However, it is present on natural sarsens on the Marlborough Downs and may originate in more than one way.
31. See B. W. Brown, *Bull. Geol. Soc. Am.* **70**, 561 (1959). Readers who object to the nonmathematical use of *p* should regard the numbers given as weighting factors indicative of my personal opinion as to the likelihood of the explanation's being the correct one.
32. See S. Piggott, *Antiquity* **32**, 235 (1958), plate 25b.
33. The experimental work to date has been inconclusive.

Science in the News

The Atomic Airplane: Its Death Has Been Mourned by Few

The project to build an atomic airplane, befitting its erratic history, was canceled earlier this month only after a mixup had produced press reports suggesting that the Administration favored the program. The reports quoted the chairman of the Joint Committee on Atomic Energy's Research and Development Subcommittee to the effect that the Administration had decided to keep moving on two competing engine approaches for the plane. The Eisenhower budget in January had recommended cutting the program in half, although without specifying which engine approach should be abandoned.

Since the chairman had just emerged from a meeting with the President, the reports carried a good deal of weight, at least for a day or two, and caused a bit of surprise, since there had been rumors of dire things in store for the airplane. Three days later the defense message was sent to Congress, engendering further surprises, since the plane had been killed after all.

The situation was curious. The President had met with the subcommittee chairman, Congressman Mel Price, of Illinois, and the chairman of the full committee, Congressman Chet Holifield, of California, in order to explain why, instead of restoring the $75 million Eisenhower had cut out of the program, as Price, in particular, had been urging, he was going to cut out another $35 million. This would leave only $25 million for reactor and materials research, which would be merged with other AEC programs. In other words, the atomic airplane, as a program, would disappear entirely, although research that it is assumed would eventually make a useful atomic plane more easily attainable would continue as part of the general effort to develop the possibilities of nuclear power.

The exact size of the rump program was not spelled out at the meeting, and perhaps had not been decided upon, but the President made clear his decision that, as he said in the message to Congress four days later, "the time has come to make a clean-cut decision." The decision would be to kill plans to build the plane.

The news was quite a shock to Price, who for several years had been the leading advocate of the plane. Pressed by reporters after the meeting about whether Kennedy was going to follow Eisenhower's recommendation and kill one of the two competing engine projects, Price said something about work on both engine approaches being continued. Apparently he was thinking of the materials and reactor research which would continue, and which might be applicable to either engine. The press naturally interpreted this in a more normal fashion: that is, that actual development work would continue on both approaches. The result was a good deal of confusion which made everyone involved, including the President, look a little silly for the moment, and annoyed everyone, including, it is said, the President.

Irregular History

The history of the whole program followed a similarly confused and confusing course, and like the final mixup, it is hard to point at any particular person or agency and say it was responsible for the confusion. Indeed a case can be made that despite the ultimate failure of the program, the numerous changes in direction of the program, which finally assured its failure, were almost all justified on the basis of the technical and military knowledge that was available at the time of the decisions. Of course a case can be made the other way, too.

From the start, the project was recognized as enormously difficult: one of the earliest of the many studies and reviews, the Lexington report in 1948, guessed that it might take 15 years and more than a billion dollars to build the first atomic plane. The central problem follows from the need for heavy shielding around the reactor in the plane.

The shield requirement, if power is constant, goes up roughly with the square of the diameter of the reactor. This means that an engine that can be put into an airplane must be driven by a very small reactor releasing a great deal of energy. This meant that, to keep the weight of the shielding down to a point where the plane could fly, reactors had to be built that could operate at temperatures about 500 percent higher than those that would be required in the first atomic submarine, another project that was begun about the same time. To keep the cost of the plane down to something that would not be entirely unthinkable these materials in the reactor had to be able to survive the intense heat and radiation for a reasonably long time.

Materials Problem

At the time the project began such materials did not exist, nor was there any way to predict with confidence how soon they could be developed. Thus the plane was a gamble from the start, in a sense that the nuclear submarine, for example, never was. It was a gamble that seemed justified, nevertheless, by the state of military technology generally: the atomic airplane would have, if nothing else, unlimited range. This could prove useful in a number of ways, but in particular it meant the plane could operate from bases in the United States to reach any place on earth. But even here there was a gamble, as the Lexington report foresaw, for by the time an atomic plane might be operational there could be alternative methods developed whereby weapons completely in control of the United States would be within striking range of any target.

The project began with studies to see if we could get within striking range, at least, of solving the basic problems of reactor technology and high-temperature materials. In 1951 things seemed promising enough that General Electric was engaged to build a power plant for an atomic plane; it would be subsonic and would fly, it was hoped, by 1957. Even then the great desirability of an atomic plane had become relatively less tempting, for we were on the verge of great advances in conventional jet engine technology which promised to

solve a part of the range problem. On the other hand, an atomic plane would be a revolutionary advance whose usefulness could not be wholly foreseen, a factor that, everyone agrees, properly encouraged work on the plane throughout its history.

General Electric studied the possibilities and chose to work on a "direct cycle" turbojet engine in which the airflow would be heated by passing it directly through the reactor. This seemed to be the simplest approach, although it had several drawbacks compared to the long-run possibilities of more complicated systems.

War Administration

In 1953 came the first reorientation: the problem had proved more difficult than had been expected, and plans to fly by 1957 had given way to plans to run a flight test of an engine, carried aloft in a modified B-36, by 1958. The Air Force Science Advisory Board advised that even this limited flight objective be canceled and GE be ordered to concentrate on basic technology. The new Eisenhower Administration, spurred by its pledge to cut the federal budget and abetted by the new Defense Secretary's lack of interest in research, at first canceled the whole program, but then reversed itself and made some money available for the research end.

Ardent supporters of the plane insist that if it had not been for this reorientation we would have a plane today, and even the most critical of the critics agree they might be right, although these critics also insist that the plane would be of such marginal utility compared with its cost that the Air Force would never have put the plane into production. If the money had been spent on putting a useless plane in the air, the critics argue, it would have taken support away from more valuable work.

At any rate, by 1953 the advances in conventional planes and the imminence of missiles had made a subsonic atomic plane seem less urgent. This, combined with the technical difficulties that were not yielding to solution, made the decision to abandon early flight understandable without blaming Eisenhower's budget cutting or Charles E. Wilson's lack of interest in research.

In 1954 Pratt & Whitney began competing with GE on developing a power plant. GE had chosen to work on a "direct cycle" engine, in which the air would be heated by passing it through the reactor. This had the advantage of over-all simplicity, but because air is a poor absorber of heat, a large volume of air had to be passed through the reactor in order to absorb enough energy to produce satisfactory thrust in the engine. This meant that the reactor could not be built as compactly as in an "indirect cycle," where the air is heated by circulating liquid metal from the reactor to radiating elements in the engine. The relative compactness of the reactor is an important point since the shielding requirement goes up sharply with an increase in the diameter of the reactor. GE had considered this before choosing the direct cycle, but decided to work on the direct cycle because it offered an easier approach to an already very difficult problem.

What made the Defense Department undertake a second engine approach at this time was the development of a new type of reactor which gave promise of providing the sought-after combination of long life and high intensity, but which could not be used in a direct-cycle engine.

Supersonic Bomber

In 1955 came a new orientation: the Air Force drew up plans for the 125-A weapon system, a nuclear bomber augmented by chemical engines that would allow the plane, when it neared enemy defenses, to raise its speed to 2000 miles an hour. A prototype was to be tested in 1959. In 1956, the 125-A program was canceled and the over-all program reoriented toward research. The speed-up in 1955 was understandable considering the unquestionable value of the 125-A system, which would outperform any manned bomber available or planned. This objective seemed worth gambling on in light of reports of rapid progress on the direct cycle and good progress on the indirect cycle. The decision was no doubt helped along by the Air Force's interest in airplanes.

The missile program by 1955 was the top priority program in the Defense Department, and missiles threatened to make manned aircraft obsolete unless there were startling improvements in aircraft. There is a parallel here with the pre-war Navy's reluctance to part with battleships in favor of aircraft carriers, but the parallel is one of psychology. It does not necessarily follow that the manned-airplane generals will be proved as wrong as the battleship admirals.

Within a year, though, it was clear that the supersonic plane was based on a wholly unrealistic appraisal of the state of reactor technology, and the program was reoriented again toward research, with no target date for a first flight.

The indirect cycle was almost completely deprived of money after the reactor design it was to use proved unadaptable to the shocks and vibrations of a flight engine. Yet progress on the reduced program was good, and gradually the indirect cycle began to compete on equal and then more-than-equal par with the direct cycle. Pratt & Whitney had started later than GE and had gotten less than a third of the money, but it had concentrated almost throughout on the basic work; GE had always pressed its directives to the limit in order to interpret them toward an early flight objective. By the time, in mid-1960, when the Defense Department told the House Appropriations Committee that a decision was to be made soon to kill one or the other approach, there was no question that GE was the likely candidate for oblivion: GE was further ahead, but Pratt & Whitney was not only showing promise of catching up, but the indirect cycle all along had been recognized as the approach with greater long-range possibilities because of its lighter shielding requirements.

On the assurance that one engine approach would be killed within a few months, the Appropriations Committee turned down, by a vote of 19 to 18, a motion to cut the program in half then and there. By now Representative Price was almost the only strong advocate in Congress for an early flight program.

But the decision to decide was put off: by late fall of 1960 the Air Force, with the support of the Department of Defense and the President's science advisers, was prepared to take the indirect cycle, although it would prefer to avoid making a choice until 1962: In 1959, specifications had been given to the contractors for a plane that, while not militarily useful itself, would be good enough to be used as a prototype from which to draw specifications for several possible planes which might be desirable in the late 1960's. GE could meet these specifications more certainly than Pratt & Whitney, where work was still at an earlier, and therefore less predictable stage of development. GE was nearly ready to ground-test a prototype power plant; Pratt & Whitney could not do so

until 1962, perhaps 1963. The Pratt & Whitney power plant would require around 50,000 pounds less shielding than the GE plant.

The decision was made more difficult when John McCone, the outgoing chairman of the Atomic Energy Commission (it was a joint AEC-AF project) turned out, apparently much to the surprise of the Air Force, to favor the direct cycle: he doubted that Pratt & Whitney could solve, within a reasonably predictable time, the mechanical problems involved in pumping several thousand pounds of molten metal, under high pressure, through the network of 18 miles of tubing connecting the reactor with the radiating elements in the engines.

Eisenhower, in the budget presented just before Kennedy took office, recommended the program be cut to one engine approach, but without specifying which.

Kennedy's Decision

The new Defense Secretary told Kennedy that, in the light of advancing military technology, he didn't think even supporting the more promising indirect cycle approach could be justified, for he didn't believe that the operational planes that might be developed from the first prototype, now vaguely scheduled for 1965, would be valuable enough to justify the several billion dollars that would be required to produce and maintain even a small fleet.

There was a conflict of views on this, but Kennedy decided to support McNamara, cutting the program back to $25 million for reactor and materials research. But most of the money, it later became clear, would be used to support work relating to the indirect, rather than the direct, cycle. So although both approaches have been officially killed, the indirect cycle remains at least half alive, and there is a fair chance we will be hearing more of the atomic airplane within another year or two.

Meanwhile the most general reaction was this one, from a man who had as much reason as anyone to regret the outcome. He thought the decision was wrong, of course, but "we've had a lot of little empires building up in the Defense Department for 15 years, and someone had to step in and do something about it. This one hurt me, but I have to give Kennedy credit. We've finally got someone who's willing to make tough decisions."—H.M.

News Notes

Element 103 Created and Identified by Berkeley Research Group

A team of scientists at the Lawrence Radiation Laboratory, operated for the Atomic Energy Commission by the University of California, have created and identified a new element, number 103 on the periodic table.

The Berkeley researchers have suggested the name "lawrencium" (chemical symbol, Lw) for the new element, in honor of the late Ernest O. Lawrence, Nobel prize winner, inventor of the cyclotron, and founder of the laboratory which now bears his name.

The discovery was made by nuclear chemists Albert Ghiorso, Torbjorn Sikkeland, Almon E. Larsh, and Robert M. Latimer.

The scientists performed their experiments with the heavy-ion linear accelerator (HILAC), one of the major tools of nuclear research at the Lawrence laboratory.

The first evidence for the production of the element 103 isotope was achieved on 14 February 1961. Workers spent the next two months in confirming the February results. Attempts to produce element 103 span almost 3 years; the final 6 months were devoted to especially intensive experimentation.

The element 103 isotope is the first to be discovered solely by nuclear methods. No chemical techniques were used in its identification.

The new element was synthesized by bombarding a target consisting of three-millionths of a gram of californium (element 98) with boron-10 or boron-11 nuclei having energies of about 70 million electron volts.

The Berkeley scientists deposited californium over a circular area 0.1 inch in diameter on nickel foil 50 millionths of an inch thick. This target foil was enclosed in a container filled with helium and placed in front of the highly concentrated beam of the HILAC.

When a californium atom captured a nucleus from the beam, a new nucleus was instantaneously formed, several neutrons were lost, and the resulting nucleus flew out of the target. This nucleus was slowed down through collision with helium atoms and was attracted to a thin copper conveyor belt. The belt was periodically pulled a short distance, to place the collected atoms of element 103 in front of a series of silicon crystal detectors.

The silicon detectors recorded a maximum of five events per hour in which alpha particles of 8.6-Mev energy were emitted by atoms of element 103. No attempt was made to identify the resulting daughter atoms of mendelevium (element 101).

Final proof of the presence of element 103 was made through a series of experiments which ruled out the possibility that isotopes of element 102 or element 101 might have produced the 8.6-Mev alpha particles. The half-life of the isotope of element 103 was determined to be about 8 seconds.

The new isotope is thought by scientists to have a maximum atomic weight of 257, although further research will be required before this can be established conclusively.

The scientists pointed out that the actinide concept, elucidated by Glenn T. Seaborg (University of California), chairman of the U.S. Atomic Energy Commission, indicates that element 103 should be the last of the actinides to be discovered. The actinide series consists of 15 elements with atomic numbers 89 through 103; they all have similar properties. Element 104 should have chemical properties different from those of all the transuranium elements that precede it.

According to one scientific theory, element 103 is an element that was formed at the birth of the universe but decayed out of existence in a matter of weeks.

Scientists in the News

A number of scientists from overseas will be in the United States to participate in a symposium on the biology of the trachoma agent, to be held 26–27 May in New York, under the sponsorship of the New York Academy of Sciences. They include **H. Bernkopf**, Hadassah Medical School, Hebrew University, Jerusalem; **L. H. Collier**, Lister Institute of Preventive Medicine, London; **J. H. S. Gear**, South African Institute for Medical Research, Johannesburg; **N. Higashi**, Institute for Virus Research, Kyoto University, Kyoto; **E. W. Hurst**, Imperial Chemical Industries, Ltd., Macclesfield, England; **B. R. Jones**, Institute of Ophthalmology, University of London; **J. Litwin**, Statens Seruminstitut, Copenhagen; **Y. Mitsui**,

Department of Ophthalmology, Tokushima University School of Medicine, Tokushima, Japan; and **F. Maxwell-Lyons**, Division of Communicable Diseases, World Health Organization, Geneva.

Laurence L. Quill, head of the department of chemistry at Michigan State University since 1945, has resigned from that position to devote more time to writing and research and to his duties as head of the university's mathematical and physical sciences division. Max T. Rogers, professor of chemistry, is acting head of the chemistry department.

R. Gaurth Hansen, head of Michigan State University's former department of agricultural chemistry, has been named head of the university's new department of biochemistry, comprised of the department of agricultural chemistry and the biochemistry section of the chemistry department.

Edward J. Forrest, associate dean of the University of Illinois College of Dentistry, has been appointed dean of the University of Pittsburgh School of Dentistry, effective 1 September.

Randall T. Chew III has succeeded Thomas Clements as curator of mineralogy at the Los Angeles County Museum. For the past 4 years Chew has been assistant geologist and mining geologist with the Northern Pacific Railway, St. Paul, Minn.

Homer F. Marsh, dean of the University of Miami School of Medicine at Coral Gables since 1954, will succeed O. W. Hyman as vice-president of the University of Tennessee (Memphis) in charge of the medical units. Hyman will retire on 1 July; Marsh will assume his new position at a date to be set later.

Two new department chairmen at Rensselaer Polytechnic Institute have been appointed, effective 1 July.

E. C. W. A. Geuze, professor of soil mechanics and foundation engineering, will become chairman of the department of civil engineering. He succeeds Lewis B. Combs, who retired last year as professor emeritus.

Hillard B. Huntington, associate head of the department of physics since 1953, will become chairman of that department. He has been on a year's leave, as visiting professor of metallurgy and solid-state physics at Yale University. He succeeds G. Howard Carragan, who will retire 30 June.

Leo G. Horan, formerly chief of the cardiology section of the Veterans Administration Hospital in New Orleans and assistant professor of clinical medicine at Tulane University, has joined the staff of the University of Tennessee College of Medicine as an associate professor in medicine.

On 1 April, **Matthias Stelly**, former professor of soils at Louisiana State University, became executive secretary of the American Society of Agronomy, which has headquarters in Madison, Wis. He succeeds **L. G. Monthey**, who had held the post since 1948 and who is now extension resource development specialist with the University of Wisconsin.

In a recent ceremony at the University of California's Lawrence Radiation Laboratory, Livermore, Navy Distinguished Public Service Awards were presented to **Harold Brown**, director of the laboratory, and **Edward Teller**, professor of physics. In addition, the Livermore Laboratory earned a Navy Certificate of Merit. The awards were made for outstanding contributions by the laboratory and by Brown and Teller to the success of the Polaris missile program.

George Pheler, professor of solid-state physics at the University of California, La Jolla, has been awarded the 1960 American Physical Society Prize of $2500 for developing a method of examining the structure and behavior of atoms in semiconductor materials. The award is sponsored by Hughes Aircraft Company of Culver City, Calif.

Robert Shankland, Ambrose Swasey professor of physics at Case Institute of Technology, received the second Case Achievement Award at a recent convocation at the institute. He was cited for services to Case during the years since 1929, when he received his bachelor of science degree from the institution.

Hans E. Thalmann, of the department of geology at Stanford University, has returned from an 18-month teaching assignment for the International Cooperation Administration at the Escuela de Geologia of the University of Chile, Santiago.

Roy L. Whistler, Purdue University biochemist, has been appointed chairman of the university's newly created Institute for Agricultural Utilization Research. The institute will promote, develop, coordinate, and conduct basic scientific research leading to expanded industrial uses for agricultural products.

Robert S. Platt, professor emeritus and former chairman of the department of geography, University of Chicago, has joined the staff of the department of geography at Ohio State University as Mershon Distinguished Professor for the spring quarter. He will teach a course on the history of geography and a seminar on tropical development.

M. T. I. Cronin, former head of the department of pathology and toxicology at the Schering Corporation, Bloomfield, N.J., has joined Woodward Research Corporation, Herndon, Va., as pathologist.

Julian Donoso-Torres has also joined the Woodward staff. He was formerly soil chemist with the United Fruit Company's Division of Tropical Research in La Lima, Honduras.

Recent Deaths

Matthew A. Hunter, Troy, N.Y.; 72; metallurgist and dean emeritus of Rensselaer Polytechnic Institute, where he served for 41 years; retired in 1949; founded and headed the department of metallurgy engineering; 24 Mar.

Khachatur Koshtoyants, Moscow, U.S.S.R.; 60; internationally known physiologist at Moscow State University, where he was engaged in research for more than 30 years; author of *Comparative Physiology of the Nervous System*, a monograph that has been recommended for the 1961 Lenin Prize; 3 Apr.

Donald S. Rawson, Saskatoon, Saskatchewan; 56; professor and head of the biology department at the University of Saskatchewan; internationally known limnologist; 16 Feb.

Masao Tsuzuki, Tokyo, Japan; 68; an authority on radiation diseases and head of the Tokyo Red Cross Hospital; was Japan's representative on the United Nations scientific committee on the effects of atomic radiation in 1957 and 1958; was active in the World Health Organization and in the movement to ban nuclear tests; specialized in treating survivors of the Hiroshima and Nagasaki atom-bomb blasts; 5 Apr.

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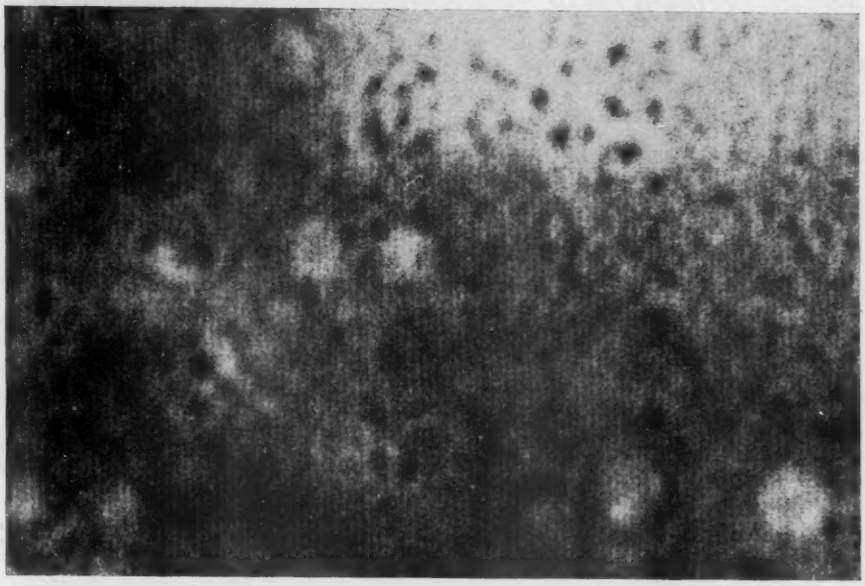

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Rationalism versus Empiricism in Cosmology

The temptation to substitute logic for observation is peculiarly hard to resist in astronomy.

G. C. McVittie

The four books which I propose to discuss in this article were all written by British scientists and are intended to inform the general public. Books of this kind are increasingly widely read today, and they largely determine the view of astronomy, and of science generally, which is held by the man in the street. Indeed, the opinions of professional astronomers are not uninfluenced by them. I have therefore thought it worth while to analyze, in more detail than is customary in reviews, the contents of these four works. I shall frequently employ quotations, the sources of which will be specified as clearly as possible. This I do because I hope the reader will look up the originals and decide for himself if the use I make of these quotations is or is not justified.

I begin with a rapid survey of the contents of the four books. Chapters 1, 3, and 4 of Hermann Bondi's **The Universe at Large** (Doubleday, New York, 1960; 154 pp.; $0.95) contain an account of the author's view of the nature of scientific theory, of the expansion of the universe, and of theories of cosmology with tests thereof. Chapters 2 and 5 through 12 deal with selected topics in astronomy, including Olbers' paradox on why the sky is dark at night; the internal constitution of stars; interstellar gas clouds; the earth's radiation belts, its motion and magnetic properties; Newtonian gravitation, celestial mechanics, and the theory of the tides.

Rival Theories of Cosmology [by H. Bondi, W. B. Bonnor, R. A. Lyttleton, and G. J. Whitrow (Oxford University Press, New York, 1961; 60 pp.; $2.25)] contains the substance of talks made on the BBC by Bonnor, Bondi, and Lyttleton, together with a discussion session among these speakers under the chairmanship of Whitrow. The topics covered are the cosmology based on Einstein's general relativity (Bonnor), the steady-state theory (Bondi), and a variant of that theory in which it is assumed that the electric charges on protons and electrons do not exactly balance each other (Lyttleton).

Fred Hoyle's **The Nature of the Universe** (Harper, New York, 1960; 141 pp.; $3) is a revised edition of the book with the same title which he published in 1950. The first two chapters contain a certain amount of descriptive material on the solar system, the sun, and the stars. Then come three chapters on the origins and ultimate fates of stars and on the origin of the solar system. These chapters are followed by an account of the expanding universe from the standpoint of the steady-state theory. The last chapter is devoted to such topics as man's place in the universe and the nature of religion.

Finally, Reginald O. Kapp's **Towards a Unified Cosmology** (Basic Books, New York, 1960; 303 pp.; $6.50) is a book of a somewhat different character, for it contains a complete exposition of the author's doctrine. This is based on certain a priori principles among which is found the notion that matter is created, and is also disappearing, without cause. The book is mainly devoted to a justification of these principles, to the description of the author's ideas on the origin and evolution of galaxies, and to a new theory of gravitation. How stars are formed and why stars and galaxies rotate are questions to which answers are also provided.

The authors belong to different schools of scientific thought. Bondi, Hoyle, and Lyttleton are advocates of the steady-state theory of the universe, Bonnor is a supporter of Einstein's general relativity, and Whitrow may here be thought of as a neutral referee. Kapp has his own personal views, though he tries to draw liberally from both the steady-state theory and general relativity.

The author is head of the department of astronomy, University of Illinois Observatory, Urbana.

Views of Astronomy and Cosmology

Let us now return to a more detailed consideration of *The Universe at Large* and *Rival Theories of Cosmology*. The chapters of the first work fall into two different groups. In chapters 2 and 5 through 12 as much stress is laid on the results of observation as on those of theory, and conclusions are presented with due qualifications. The statements it contains would, in the main, be agreed to by most astronomers. The presentation in these chapters has another virtue, namely, that a great deal of thought has evidently been given to finding excellent simple analogies for the recondite subjects under discussion. The effortless prose should not deceive the reader; beneath it lies much hard work in discovering the best illustration.

In chapters 1, 3, and 4, which may profitably be read in conjunction with his contributions (chapter 2 and the debate in chapter 4) to *Rival Theories of Cosmology*, Bondi is concerned with making a case for the steady-state theory, which he and T. Gold originated. The starting point is Karl Popper's dictum that a scientific theory can never be proved to be true but, instead, that certain theories can be proved to be false by an appeal to observation. This is probably unimpeachable doctrine, but Bondi goes on to write as if the excellence of a theory were measurable by the rapidity and ease with which it can be so disproved. Thus on page 45 of *Rival Theories* we have: "I certainly regard vulnerability to observation as the chief purpose of any theory." Or on page 35 of *The Universe at Large* we may read that one of the chief tasks of a theory is "to suggest methods of shooting down the theory." At this rate we should be justified in inventing a theory of gravitation which would prove that the orbit of every planet was necessarily a circle. The theory would be most vulnerable to observation and could, indeed, be immediately shot down.

The steady-state theory is built up from the assumption that the universe is uniform in space and time. This means that an observer located any-

where in space and making his observations at any moment in the history of the universe sees the same world-picture as we do now from the earth. It is also accepted that the universe is expanding; this will lead to a steady diminution of the average density of matter and therefore to a change in the world-picture as time proceeds. To prevent this from happening, matter must be created out of nothing at just the right rate to restore the balance and keep the density constant. Once created, however, matter cannot be correspondingly annihilated though it may presumably be converted into energy.

In defending this theory, Bondi is not entirely consistent. For example, on page 23 of *The Universe at Large* he is discussing the possibility that the red shift phenomenon might have some other interpretation than a velocity of recession. Defenders of this view have "argued that all our interpretation was based on our local knowledge of physics, and that unknown effects might well occur in the depth of the universe that somehow falsify the picture that we receive. Nowadays, we have little patience with this type of argument." Now on page 17 of *Rival Theories* it is said that the creation of matter is "a major infringement of present formulations of physics," formulations that derive from our local knowledge. The creation of matter is admittedly not observable in terrestrial-scale experiments. It is therefore precisely one of those unknown effects, brought in to deal with the depth of the universe, with which we have little patience when they are invoked to explain the red shift.

The steady-state theory also asserts that galaxies are continually forming out of the newly created matter and that this process has been going on for an infinitely long time. There must therefore be somewhere in the universe infinitely old galaxies. Throughout their lifetimes matter has been created within them and in their neighborhoods. Their gravitational fields will have prevented the escape of this matter, and therefore the old galaxies will by now be enormously more massive than our own or than the galaxies which we do observe. These old massive galaxies are not observed and so have to be disposed of. This is done (page 46 of *The Universe at Large*) through the expansion effect. In fact "expansion to regions hard to see is the process of death" of a galaxy. Obviously this is a way out of the difficulty, provided that the observer is always located in a recently formed galaxy. An observer in an old massive galaxy would see it quite clearly around him. He would also conclude that his position in the universe was a peculiar one because the galaxies around him would, for the most part, be far less massive than his own. Therefore his world-picture would not be identical with the one which we, for example, obtain from the earth. This conclusion apparently contradicts the fundamental postulate of the theory, which states that all world-pictures are equivalent to one another, unless some device is introduced for killing off all possible observers on ancient galaxies.

In these four books there are a number of statements about Einstein's general relativity which are either incorrect or are true only with essential, and unstated, qualifications. Take, for example, the remark by Bondi on page 36 of *The Universe at Large* that the assumption of the large-scale uniformity of the universe "has to be added" to general relativity in order to produce a cosmology. In fact the assumption is added merely to make the mathematics simpler; there is nothing in general relativity which forces us to make it. Indeed, studies are at present in progress that aim at dispensing with the assumption. They are referred to briefly by Bonnor on page 9 of *Rival Theories of Cosmology*.

Illustrations of statements needing qualification will be found on page 37 of *The Universe at Large* and on page 119 of *The Nature of the Universe*. It is said that certain models of the universe deduced from general relativity involve an initial "nuclear explosion" or a "big bang" which initiates the start of the expansion. What general relativity itself demonstrates is that, if we push the assumption of uniformity too far back into the past, this idealization will have the following consequence. It will appear that the motion of expansion of the universe has begun from a state in which all the matter in the universe was concentrated at a single mathematical point. But general relativity predicts no nuclear explosion, big bang, or instantaneous creation, for that matter, as the cause of the start of the expansion at that moment. Such notions have been woven round the predictions of general relativity by imaginative writers. The correct interpretation of the initial instant, as a product of an oversimplified mathematical treatment of a complex problem, is well brought out by Bonnor in chapter 1 of *Rival Theories*. In this book we find him, a doughty warrior outnumbered two to one, stoutly maintaining the general relativity point of view that scientific cosmology should be based on the laws of physics as we know them from experiment and observation rather than on hypotheses and principles laid down a priori.

The chapter headed "Tests in cosmology" in *The Universe at Large* throws a clear light on the attitude of the advocates of the steady-state theory toward astronomical observation. An argument against the theory would be the detection of some change in the properties of galaxies which depended systematically on distance. This would really be a time variation, for the further away a galaxy is, the further back in time the light by which we now see it departed from the galaxy. In pointing this out, Bondi fails to mention that an effect of this kind has been suggested with regard to the nature of clusters of galaxies (*1*). Another test mentioned is the character of the increase in the number of galaxies with distance. Again there is a failure to mention the fact that the observations we do possess—those that refer to galaxies that are radio sources—fail so far to conform to the predictions of the steady-state theory (*2*). Two other tests suggested are possible only if we accept a certain theory of the formation of galaxies and a theory of the formation of the elements, respectively. These are not observational tests but rather examinations of the concordance between the steady-state theory and certain other theories. Finally there lies under the statement (page 55) "there are many other tests which are too technical to be considered here" perhaps the most significant test of all. This refers to the way in which the red shift varies with increasing distance. The often repeated remark in these books that the red shift of a galaxy is proportional to its distance is only a rough first approximation. The observational work carried out up to 1956, and extended during the past 5 years, reveals that the relationship is a more complicated one. This in turn can be shown to imply that the acceleration of the expansion is negative at the present time. The steady-state theory, however, predicts that the opposite is the case, namely, that the acceleration must always be positive. This difficulty is referred to indirectly by Lyttleton (pages 22 and 23 of *Rival Theories*), who dismisses it by arguing

in effect that to rely on observation alone is to adopt the wrong attitude in science.

Lyttleton's main contribution to *Rival Theories* is an account of a theory which he and Bondi had just published in the technical literature (*3*). It is postulated that the positive charge on a proton is slightly in excess of the corresponding negative charge on an electron. If this is so, then a hydrogen atom will have a slight positive charge. If, again, the universe is thought of as a highly rarefied cloud of hydrogen gas, matters can be adjusted so that the electrical repulsion between the charged hydrogen atoms counteracts gravitational attraction and produces a general expansion of the universe. But to achieve this result it is also necessary to modify the well-established Maxwell equations of the electromagnetic field. The modification proposed implies that the outward flow of charged hydrogen atoms will produce no magnetic field (page 27, *Rival Theories of Cosmology*). Be this as it may, the theory has satisfied Bondi's criterion for a good theory: the experimentalists "shot it down" almost immediately. Hillas and Crankshaw (*4*) showed that the required excess positive charge for the proton as compared with the electron was not present and, further, that the experiment could have detected the excess even if it had been 1/100 as large as Bondi and Lyttleton needed for their theory.

We may conclude this discussion of our first two volumes by remarking on Whitrow's skill as an impartial chairman. If, following some cataclysm, *Rival Theories* were the only book published in the 20th century to survive the disaster, the archeologists of the future would have the greatest difficulty in deducing from this volume what his views on cosmology really were. Happily for us, he has recorded them elsewhere (*5*).

Hoyle and the Universe

The first edition of *The Nature of the Universe* appeared in 1950 and consisted of the texts of a series of lectures delivered over the BBC shortly before. It is perhaps one of the most widely read popular books on astronomy and cosmology; it is very well written, and Hoyle states regarding the second edition, "I have tried not to change the general style and plan of the book." The method of exposition is to state conclusions with an air of absolute certainty. The conclusion may be one that is well founded in observational and theoretical work or it may be a highly questionable idea that has occurred to the author or to one of his colleagues. It is also a very interesting exercise to compare the present edition with the original one. Let me give some illustrations of the author's method. ["I" refers to the first edition (Mentor Books, New York, 1950); "II" similarly refers to the present edition.]

In II (chapter 1), Hoyle describes the earth, the planets, and the satellites of the solar system. At one point he refers to the theory that the so-called craters on the Moon are the results of meteor impacts. In this theory, the maria, which are the large dark areas on the Moon almost devoid of large craters, are thought to be the consequences of impacts by very large meteors indeed. Considerable areas of the Moon were liquefied and, on congealing again, formed relatively flat plains. No reference to this interpretation is made by Hoyle; he quotes instead an idea "pointed out [to me] by my colleague, Gold" (I, page 19; II, page 13). This is that the maria represent regions covered by an extremely deep layer of dust which has buried all the peaks and craters. We see therefore the top of a sea of dust. Unfortunately this explanation ceases to be plausible as soon as we look at a large-scale photograph of the surface of one of the maria. It then leaps to the eye that we are contemplating a plain of rock covered with small sharp irregularities, tiny craters, cracks, and so on.

Or take the discussion of the formation of the earth. In I (page 86) the answer to the origin of the "complicated rare material out of which the earth and the planets are made" could be given "in two sentences." The explanation was that the earth and the planets were formed out of a small remnant of the gases that resulted from the supernova explosion of a star. This star had been the sun's companion in a binary system; it was ejected from the neighborhood of the sun during the explosion and is now unrecognized. But now turn to II (chapter 5) which "has been entirely rewritten." This is certainly true, for there is no longer any mention of the sun ever having had a companion which suffered a supernova explosion. Instead the sun in its early history developed a gaseous ring, angular momentum was transferred to the ring by the action of magnetic forces, and some of the gas condensed into the planets. In support of this view we have (II, page 98): "astronomical observation provides ample evidence that rapidly rotating stars do in fact grow disks of gas in this fashion." My own impression of the professional literature is that the "ample evidence" is principally based on the interpretation of the spectrum of one star, Pleione (*6*), and that Hoyle himself is uncertain whether a disk or a shell of gas is in question (*7*). That the spectral peculiarities of one star, or even of a small number of stars, may be interpreted by postulating a disk of gas hardly constitutes the ample evidence provided by astronomical observation which would be required as a starting point in a theory of the origin of the solar system. Equally strange is the statement (II, page 101): "the overwhelming proportion of the water [in the whole planetary material] is now to be found in the great planets, not in the small inner planets." I was under the impression that the only planet for which we had indubitable observational evidence for the presence of water in substantial quantity was the earth, a small inner planet. Hoyle here appears to be drawing on an early theoretical model of the planet Jupiter proposed by Wildt in which a large shell of ice is postulated in its interior. A recent survey article on the subject of planetary interiors (*8*) makes no mention of water or ice in the interiors of Jupiter and Saturn, which are regarded as largely composed of hydrogen and helium with unspecified heavy elements in their small central cores.

In summary, one can say that, without warning his readers, Hoyle selected one of the many theories of the origin of the solar system in 1950 and another one in 1960. On each occasion the selected theory is presented as if it were the only possibility (*9*), were strongly supported by the evidence, and were generally agreed to by astronomers.

When Hoyle is writing on the internal constitution of the sun and stars, to which he has himself made great contributions, he has an excellent and well-balanced tale to tell (II, chapter 2). Energy generation, chemical composition, the difference between an ordinary "main sequence" star and a red giant, and other relevant matters are all clearly and succinctly described. However, in the next two chapters on the origin, the future, and the final

state of a star, we have a return to the technique of presenting personal opinions as if they were the generally agreed-on results of astronomical research. In 1950 (I, page 59) a main element in star development was the process of accretion by which stars swept up the interstellar material. This process has inexplicably disappeared from the picture in 1960. Stars now condense out of the interstellar gas in groups and not singly, an idea presented in the words (II, page 71) "the more dramatic answer is the correct one. Stars are born in groups." In fact, this is a useful hypothesis on which astronomers are now working; certain stars may well be born in groups, but it would be a bold astronomer who would assert that he knows that *all* stars are produced in this way.

Hoyle is a supporter of the steady-state theory of cosmology, and his account of it (II, chapter 6) is similar to Bondi's. However, he adds two new ideas. One is that the newly created material "produces a pressure that leads to the steady expansion" (I, page 114; II, page 126). In contradiction to this, Hoyle proves in his technical papers (*10*) that the expansion of the steady-state universe is not contingent on the presence of a pressure and indeed takes place under the fundamental assumption that the pressure is always zero. The second idea is that the creation of new matter arises from a field which "you must think of as generated by the matter that exists already" (II, page 123). The notion is defended by analogy with a gravitational field, but there is no true resemblance. The gravitational field is a mathematical expression of the fact of astronomical observation that, wherever matter is observed, it appears to exert a gravitational force on other matter. There is no experimental or observational evidence of any kind which suggests that the mere presence of matter is responsible for the creation of new matter out of nothing.

The treatment of numerical values between the first and the second editions is also very interesting. Usually the text is not modified in any way, but the new value is simply inserted into the slot previously occupied by the old number. Here are some examples, the number found in the first edition being followed in brackets by the one occurring in the second. "The birth of the earth, which, as I shall show in a later chapter, occurred about 2500 [4500] million years ago" (I, page 18; II, page 11). "To give a numerical estimate, I would say that rather more than 1 [1000] million stars in the Milky Way possess planets" (I, page 30; II, page 25). "Calculation shows that the supply of hydrogen in the sun will last for about 50,000 [5,000] million years" (I, page 43; II, page 41). "A large matchbox full of it [the interstellar gas] would contain only about 10,000 [100] atoms" (I, page 54; II, page 67). "The speed [of the sun] is in the neighborhood of 1,000,000 [500,000] miles an hour" (I, page 55; II, page 67). Yet in both editions the sun takes 200 million years to complete one revolution round the center of the galaxy. This implies that the radius of the sun's orbit was halved between 1950 and 1960, news which will startle astronomers. "The estimate [of age] for the stars of the cluster M 67 comes out at about 15,000 million years" (II, page 82). But in 1950 we have "No estimate exceeds 4,000 million years" (I, page 74). Galaxies "certainly continue out to the fantastic distance of 1,000 [5,000] million light years" (I, page 100; II, page 110). Clearly a scientist may change his mind about such matters as knowledge develops. But the helpless reader has no means of knowing why and how these alterations have been made and whether the new values have any greater degree of certainty than the original ones. His bafflement can only be increased by the statement that "the time estimates of the astrophysicist . . . are reasonably definite and precise" (II, page 82). One might suppose that some at least of these numbers were misprints were it not for the fact that this book is laudably free of obvious ones. I have noticed three only: the nearest star is distant 4¼, and not 3, light years (I, page 52; II, page 65); "changing with" (II, page 120) replaces the original "monkeying with"; and there appears to be something essential omitted from a remark of Sherlock Holmes' (II, page 93).

Cosmology by Cogitation

It is the fate of those who have a professional interest in relativity and cosmology to receive through the mails, several times a year, closely typed manuscripts containing new theories of the universe. The covering letter is fairly stereotyped and runs as follows: The author of the manuscript has found a new principle which holds the key to all, or most of, the unsolved problems of physics and astronomy. He is himself not a specialist in science and therefore has neither the time nor the required background knowledge to make full use of his important idea. He has therefore approached a number of professional scientists who, in spite of his careful explanations, have shown no interest in the new principle. The author therefore has written up his theory, demonstrated as far as he can the important consequences to which it leads, and hopes that all professional scientists may thus be led to revise their work in the light of the new revelation.

These elements will be found in the opening paragraphs of the preface to Kapp's book, set forth in more elegant language than is customarily the case. There is also a defense of the point of view that a writer who seeks to unify all science cannot be expected to be equally familiar with the details of all branches of it. This is a valid defense with the following reservations: if the writer is an engineer, like Reginald Kapp, he cannot be excused from being ignorant of the fundamental ideas of classical mechanics, or, as Kapp calls them (page 47), "simple mechanics." Nor if he makes a conclusion drawn from a more recondite theory—general relativity in this case—basic to his exposition, can he be excused from showing ignorance of the aforesaid recondite theory.

In examining a work of this type a reviewer ought to allow the author the right to have his work discussed entirely within the framework he has laid down. The scientists to whom he has spoken will have pointed out the inconsistencies with experiment and observation and with the theories that have been developed to interpret the results. These inconsistencies are evidently regarded by the author as irrelevant; otherwise publication of the theory would not have been undertaken. Let us therefore ask whether Kapp has followed his own rules in carrying out his program.

The fundamental postulate is the Principle of Minimum Assumption. On page 34, Kapp says that a minimum assumption "can be recognized by the use in its formulation of the words 'any' or 'either.' " For instance the statement that a planetary system can have any number of planets is a minimum assumption; a statement to the effect that such a system cannot contain 13 planets is not. On page 38, the principle is also defined, in part, by the statement: "In physics a generalization that is logically possible is also phys-

ically possible." Finally, on page 43, the "criterion of minimum assumption" is equated with Occam's razor, the notion that hypotheses are not to be multiplied unnecessarily.

A second basic principle is that of the Symmetrical Impermanence of matter and energy (page 43). This states that any particle of matter or quantum of energy may have existed for any length of time and, conversely, may cease to exist at any time. Matter and energy can originate at random, without cause, and, conversely, can be extinguished in the same fashion.

Lastly there is the Cosmic Statute Book, a notion which is not fully explained, reference being made to an earlier work of the author's. It appears to be a compendium of prohibitions as to what can or cannot be done in physics and to be analogous to the Statute Book of a sovereign state. In the British one, for example, it is prohibited to drive a car on the right-hand side of the road. Logically one can drive on either side, and therefore the prohibition is an arbitrary one. On page 38 it is stated that "for the physicist there is no such thing as a Cosmic Statute Book."

Let us see what use the author makes of his principles. To analyze every application would mean writing a book longer than Kapp's, and therefore only a few key examples will be noticed. Consider the statement (page 81) that the "interstellar gas must be falling on to the stars." Suppose that this is so and that Symmetrical Impermanence is false. Then matter and energy have existed for all time, and the material of interstellar gas clouds would long ago have fallen into the stars. But interstellar gas clouds are observed; therefore new matter must have been created at intervals and, since stars are not of infinite mass, matter must also have been extinguished.

Now why does the interstellar gas "fall" on to the stars? Because the stars have gravitational fields and ponderable matter must be "falling everywhere down the potential gradients of the [gravitational] fields" (page 81). This notion that the direction of the velocity of a body in a gravitational field is always parallel to that of its acceleration is a recurrent theme. It first occurs on page 45, it is basic to the theory of interstellar clouds, it is used repeatedly in chapters 10 and 11 on the formation of galaxies, and it is repeated on pages 104 and 105. However, the Principle of Minimum Assumption would state that the velocity of a body in a gravitational field can have *any* direction relative to that of the potential gradient. This fact is an elementary consequence of simple mechanics, and it is amply illustrated in astronomical observation, for example, by the motion of the earth around the sun. Kapp has here drawn on the Cosmic Statute Book for an arbitrary prohibition, namely, that a body may not move except in the direction defined by that of the potential gradient. Without this rule, his theory of the existence of interstellar gas clouds and of the formation of galaxies goes by the board.

The same can be said of his treatment of radiation pressure on pages 93–95, the result being quoted also on page 120. It is asserted that the radiation pressure on an interstellar gas cloud always acts in the direction opposite to that of the gravitational potential gradient acting on the cloud. In fact, cloud and illuminating star move in directions different from that of the potential gradient that controls their motion, namely, that of the galaxy as a whole. The force of radiation pressure on the cloud due to the star's radiation is therefore, in general, different from that of the potential gradient. Here again a cardinal prerequisite of Kapp's theory is that the Cosmic Statute Book should contain a law by which the pressure of radiation and the gravitational potential gradient are forbidden to act at an angle to one another.

Let us look now at the notion of Symmetrical Impermanence itself. On page 52 it is stated that, when matter originates without cause, it is an elementary constituent of matter that so originates and this "may be a bit of differentiated space." On page 189 we have: "the origin of matter and the origin of space occur in association and the extinction of matter and the extinction of space also occur in association." Now consider the statements on page 109, the first of which is "the minimum assumption is that every region of space has at any given moment an equal probability of being the birthplace of a particle" and the inference that "the rate of origins is constant per unit volume." The minimum assumption implies only that the probability is a function of time alone. The selection of a constant value for this function is an arbitrary restriction of the kind found in the Cosmic Statute Book. Next, again on the same page, we have: "the minimum assumption is that every particle at that moment has an equal probability of becoming extinct" and the inference that "the rate of extinctions [is] constant per unit mass." The rates of origin and of extinction are therefore asymmetrical, one depending on unit volume, the other on unit mass. This asymmetry is defended on the ground that "in a perfect vacuum there can be no extinctions, for there is nothing to become extinct." But there is space in a vacuum, and it is not clear why the extinction of *empty* space is prohibited, since space *is* extinguishable when it happens to be occupied by matter. In any case, the processes of origin and of extinction could be logically brought into agreement so that both would depend on unit volume, and both would be variable with time. The notion would then still be in agreement with the Principle of Minimum Assumption and would be symmetrical to boot. Since Kapp does not proceed in this way, he must be relying on arbitrary prohibitions in the Cosmic Statute Book. The need for these prohibitions is found in his theory of gravitation in chapter 25, where the cause of a gravitational field is stated to be the extinction of matter. Since this theory appears to be based on a denial of Kapp's own rules, it need not be discussed further here.

It is when he comes to take over conclusions from general relativity that Kapp has most frequent recourse to the forbidden contents of his Cosmic Statute Book. This volume apparently contains statements in flat contradiction to the conclusions that can logically be deduced from general relativity; it also apparently contains arbitrary selection rules as to which of the correctly deduced results are to be permitted and which forbidden. I will illustrate Kapp's method by a few examples. On page 35 it is implied that the laws of physics are no longer necessarily of the kind that assume a Euclidean space. Einstein has introduced the minimum assumption that the geometry may be non-Euclidean. In fact, Einstein did more: his minimum assumption was that the geometry of space-time is non-Euclidean. The statement on page 271, namely, "the motion [of a particle] depends, according to the [general relativity] theory, only on the curvature of space" is consequently untrue. Cases are known in general relativity in which space is Euclidean but space-time is curved, and this state of affairs determines an accelerated motion for particles. On page 72, in chapter

25, and in appendix H it is stated to be a consequence of general relativity that, if the material content of the universe is increasing, its extent must be doing so. This is false because a basic postulate of general relativity is that mass cannot be created or destroyed, in Kapp's sense, though it is convertible into energy. Moreover it can be logically deduced from general relativity that the volume of space may be increasing or decreasing while the material content remains constant in amount. This occurs in the model universes of constant positive curvature. In other words, general relativity predicts no connection between the expansion of space and an increase or decrease in its material content. The interpretation of the red shift given on page 58 and the calculations on page 112 constitute an arbitrarily selected, special case from among the logically deducible predictions of general relativity. The statements on pages 73 and 74 imply that the intensity of a light source and the intensity of gravitation both fall off as the inverse square of the same "distance." That this is untrue is one of the most remarkable consequences logically deducible from general relativity. Lastly, it is to be noticed that, whenever a mathematical argument involving mechanics is carried out, the argument presupposes that classical mechanics is applicable, that is, that space must necessarily be Euclidean. Examples occur on pages 100, 101, 112, 113, 114, 183, 184, 220, and 221 and in chapter 21. Thus conclusions drawn from these arguments are not shown to be valid if Einstein's minimum assumption is adopted; they follow in fact from the Newtonian nonminimal assumption.

It is perhaps illegitimate in discussing a theory of this type to point out where basic starting points in the discussion are contrary to observation. One only will therefore be mentioned. It occurs on pages 126, 127, and 131 where it is said that spiral nebulae rotate as though they were composed of a viscous fluid and that they are in uniform rotation. Measurements have shown that these statements are untrue; indeed the nonuniformity of the rotation, for different distances from the axis, is one of the problems to be solved in any theory of the rotation of these objects.

The foregoing examples of the nonapplication by Kapp of his own rules and of his free use of the entries in the (forbidden) Cosmic Statute Book are far from exhaustive. Indeed there is hardly a page in the book on which the reader can fail to find an instance. These internal self-contradictions may perhaps be one of the reasons for the lack of interest in his ideas that Kapp has detected among scientists.

We have seen that Kapp expressly states a principle which Bondi, Hoyle, and Lyttleton also seem unconsciously to follow. It is the dictum that what is logically possible is also physically possible. At first sight the principle appears to be quite harmless, for it seems merely to say that we must examine any idea that occurs to us. In practice, scientists are human; it is as unpleasant for them as for the next man to admit that an idea, once formulated and published, is, after all, either useless or unimportant in science. Assuredly one of the remarkable features of science is that logical possibility is by itself insufficient; one may even say that three or four hundred years ago the founders of modern science were struggling to establish this insufficiency. They had grasped the notion that the physical world had to be closely and continuously inspected if it was to be understood. The temptation to substitute logic for observation is peculiarly hard to resist in astronomy. This is because astronomical data are very difficult to come by, and the data rapidly diminish in number and accuracy as the objects we observe recede from the earth. We need only reflect on the scantiness of the information we possess about our nearest planetary neighbors, Venus and Mars, compared with the wealth of physical data which geophysicists and meteorologists can supply about the earth. Nevertheless, the fact that data may be scarce and inaccurate is no reason for failing to use them as our main guides in the formulation of theory. Perhaps I may be allowed to close this long review with one final comment. Once upon a time British science was sometimes criticized for being too empirical. During the past 30 years a number of a priori theories of cosmology, of which the steady-state theory is one, have completely reversed the trend, which is a curious and unexpected development.

References and Notes

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Social Sciences and General

The Future of Mankind. Karl Jaspers. Translated by E. B. Ashton. University of Chicago Press, Chicago, Ill., 1961. ix + 342 pp. $5.95.

The problem of this book is what Jaspers calls "the new fact"—the possibility of the thermonuclear termination of human history. What is his solution? That takes a little longer to say, but it may be said briefly that there is no "solution," if solution means some new and cleverer plan, strategy, law, or institution based upon our present thinking. It is the thinking itself which must be changed. And the change in question is not a mere shift of ideas, but a change that will involve our deepest sense of life.

The great merit of the book lies in the earnestness and humility with which Jaspers faces the new fact, unmasks spurious solutions, and searches for the attitude that would make hope reasonable. To recover reason in a situation maddening enough seemingly to defeat all reason is the principal aim of the book, but the reason which is found is hardly anything that can be encapsulated in a phrase. The reader who expects a new plan of action comparable to existing alternatives will be disappointed; but it is exactly that expectation which Jaspers regards as profoundly mistaken and which must be seen through if any hope is to be justified.

Nuclear Extinction

The first step must be to face the new fact squarely and in its depth. The new fact is not the death of an individual man or the disappearance of a nation; it is the possible extinction of humanity through its own action. But everybody "knows" this; and yet Jaspers feels we do not know it at all. "Today we see politicians whose countenance and deportment baffle us. Do they know where they stand, what they

are doing? What are they thinking of? Invariably they show us laughing faces" (page 237).

Jaspers himself presents no "laughing face." He characterizes his attitude as beyond either pessimism or optimism; it is the resolute exploration of the possible, grounded on an absolute conviction of the worth of reason, freedom, and human dignity. We do not "know" the new fact until we have been shaken to the core by it. The idea of the imminent extinction of humanity is so shocking to our practical and vital intelligence that our instinctive attitude is to interpret it out of existence. On the one hand, since it is after all a mere possibility, we adopt the attitude that it is virtually impossible; it would never really happen. Or, on the other, when we take the possibility seriously, we sink into fatalistic despair: nothing can be done, it will either come of itself or not come at all; and if it comes, then that's it, the world is bound to perish anyway.

When these attitudes are perceived as essentially blinding and despondent, we look for "concrete solutions." Is not this a problem like any other? Intelligent men will find a way out. If science has created this new fact, more science will be able to cope with it. Or, if the inhuman uses of scientific discoveries are beyond the competence of science itself either to define or to control, political means will avail. "Moral progress must catch up with science . . . ." The United Nations, disarmament, treaties, individual sacrifice, or a new balance of terror will serve to ward it off. Everyone will see that it is to no one's advantage. No one will initiate the holocaust, out of self-interest.

And yet men so seldom are guided by anything like "self-interest." Thus, we find a profound ambiguity of thought on the highest political level; at one and the same time, we have integrated human annihilation into national policy as our supreme threat, and we continue to comfort ourselves that it will never be used. What, Jaspers asks, is the meaning of a threat which we intend never to use? Or of the United Nations? For Jaspers the U.N. is magnificent in words but in fact is nothing but the institutionalization of old politics, of "the principle of falsehood." The veto unmasks the lie; it is nothing but an elaborate device of national sovereignties to preserve their sovereignty.

Coupled with the possibility of total nuclear extinction is another of an equally absolute character. Men profoundly disagree on the sort of life worth living. If the new bombs could make life biologically impossible, the new totalitarianism could make it humanly worthless. Human freedom is the very essence of human dignity, and it is the only atmosphere within which men can live lives worth living.

Freedom itself is not, in Jaspers' view, a "natural faculty," like the web-spinning instincts of spiders. It is not inherent or inevitable. It has frequently been extinguished in the past and has risen again only from external sources. But now there are no external sources; the history of the past can be altered, and the globe is essentially occupied. Gandhi's action was meaningful only within the context of the British Empire, already permissive of forces which opposed it. The crushing of the Hungarian revolt in 1958 shows what can happen now. Freedom then is inherently precarious; and it is well within the possibility of present totalitarian methods to extinguish it altogether, once and for all. Human history would then terminate in another sense: we should no longer recognize the survivors as genuinely human.

The gravity of this problem is, needless to say, hardly met by the slogan of Bertrand Russell's movement: "Better Red than dead." One of the many refreshing virtues of Jaspers' book is the total absence of such simple and sloganistic formulas. Jaspers says: "Man is born to be free, and the free life, in the sense of existence—individual life as well as all life—can be staked and sacrificed for the sake of the life that is worth living" (page 169). But: "In all these arguments for and against the final risk, it must not be forgotten that both parties reckon with certainties that do not exist: with the total extinction of mankind by the superbombs or with the total corruption of humanity under total rule. Neither decision is *sure* to destroy either human life or a life worth living. No situation is absolutely hopeless" (page 167).

Man's Work and Nature's Work

Where then is the hope? Jaspers sees it alone in the cultivation and spread of what he calls "reason." Reason is not a new solution, but rather the inner attitude and atmosphere which must control every solution if it is to be a solution. Reason is not an affair of experts, logicians, scientists, technologists, or calculating machines but rather that final honesty which seeks to clarify our ultimate and common situation. It conceives of truthfulness as "what exists only in common. The individual can not be rational by himself." It is not "to agree with everyone, but to understand his meaning, and allow him his due." It is radically open: "The very adoption of final positions is already irrational." And, although radically open, it is yet the very principle of decisiveness: "The paradox of reason is to be open and to preserve freedom while binding itself so as to lead to a decision in the concrete historical moment." It is intolerant of intolerance alone.

Reason cannot be taught as though it were a doctrine, but it can be elicited by example and by appeal. At least such is the inherent hope of every reasonable man. Reason keeps its eyes on the whole, although that whole can never be adequately grasped. It is mere intelligence that loses sight of final ends, of life itself, of the totality of conditions of life in its pursuit of particular, realizable goals. It is reason that must control as well as animate mere intelligence if the great common disaster is to be held off.

Reason, for its part, is ultimately animated by a philosophic faith in Transcendence. We are human only through faith in what transcends the merely human, in God if that term could be stripped of all dogmatic connotations. We do not and cannot *know* God or God's will; but the faith and hope of human reason, its earnestness, honesty, search for the whole, and its inherent desire for freedom and communication are themselves manifestations of a philosophic faith in Transcendence. The impatience of reason with every form of dogmatism, whether scientific or religious, is a consequence of the perception of reason that Transcendence cannot be grasped, and yet it is the final value and reality.

In a word, then, Jaspers feels that human existence is facing a historically unique extremity; it is the magnitude of this extremity which, if explored, can possibly awaken us to the meaning of the human life that is now threatened. Only in the most earnest realization of the extremity will we recover that reason which is the basic condition of any possible solution. We must therefore become more human if we are to remain human at all.

The present extremity is not hopeless, since it is not a necessary event in nature but only a possibility, and a possibility which lies strictly within

man. "Atomic doom is not a necessary process that comes over us and has to be accepted. Every step depends on men who take it on the road to disaster. . . . We must recognize the difference between man's work which is up to us, and the work of nature which we can master only to a degree. We must not fail in what is up to us, by submitting to fate from the outset" (page 231). What we do with the new fact then is up to us—that is, up to our freedom, or, up to our ultimate reason. Hence the radical importance Jaspers places upon the development of reason in the individual and in society.

It is impossible in this brief résumé to give much of an idea of the inherent realism of Jaspers' thought; his analyses of the end of colonialism, of the corresponding changes in national economies, of truthfulness in politics, and of the questions of national sovereignty, disarmament, and the possible elimination of war as such, all show concretely his perception of the dilemmas involved in all partial solutions. Nothing will work reliably unless it is undertaken in the spirit of reason and freedom. But if the spirit of reason is dominant, then indeed, we cannot hope to continue as before; reason is not a new and more sophisticated interpretation of our old political life; if it is reason, then it is honest, and the old political life must be transformed profoundly and genuinely from within.

In all of this Jaspers does not speak from on high, but humbly presents the book as one contribution to the discussion of this most extreme of all problems. It will disappoint those who expected a new, unthought-of, and more clever plan. But it is written exactly against such modes of thinking. It will also disappoint those who expect a "philosophical" justification for their own religious, political, or scientific dogmatisms. Instead, the book itself is an example of the reason Jaspers is urging; it presents an earnest and relentless analysis of the situation, always to reveal at the end the basic condition on which solutions depend: the rational faith which must endlessly be cultivated and pursued.

Perhaps the book is too long and too repetitive; but Jaspers' point cannot be made quickly or through brilliant single phrases. E. B. Ashton has translated the book beautifully and idiomatically; there are only very occasional oddities of expression—"glacis," "cognoscibilities."

My own final feeling was that too much of the total analysis rests upon a contrast between "mere intelligence" which is "unreliable" and "reason" which is honest reliability itself. Mere intelligence cannot be guaranteed to assure us of the continuation of the human race. All strategems, institutions, laws can be misused by those not thoroughly penetrated by the spirit of reason, which, for its part, must live in the continuing awareness of extremity and must be animated by good will. But if we must count on such a magnificent inner transformation in mankind for human beings to survive at all, then indeed are we not already lost? For on the one hand, such honest reason is already the basis of the thinking of responsible statesmen, who do not *all* present "laughing faces"; and on the other, if we can only survive through the spread and intensification of reason, then indeed the future of mankind is staked exactly on those men of reasonable good will who would not endanger it in the first place.

Let us then imagine we have done our uttermost to cultivate the reason of extremity, the honesty and clarity about our total situation which Jaspers calls for; as such imagined reasonable men, what do we do now?

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An Elementary Russian Science Reader. I. B. Faden. Methuen, London, 1960. 62 pp. $1.

This unpretentious reader could be a happy discovery for those who, having mastered the essentials of Russian grammar, would like to start reading scientific Russian but do not yet feel up to tackling the "solid stuff."

Included are 46 very brief passages on a variety of topics involving elementary concepts of general science as presented to Russian school children. While one could hardly expect such contents to be excitingly interesting for adult scientists, many will appreciate the opportunity to amass a good deal of basic vocabulary, without pain, while they enjoy the satisfaction of finishing a given reading at one relatively short sitting.

Each passage is accompanied by a word list, and there is a useful word index at the end. The translations in the word lists are extremely well done. However, го́рная поро́да (page 10) should be translated as "rock," and the Russian expression for "until such time as" should be expanded to до тех пор, пока́ . . . не. It seems unfortunate that some eight words are not translated in the word lists of the passages where they first occur. These words are translated in later word lists. Incidentally, those who want to understand everything will undoubtedly need to use a dictionary, but perhaps not too often.

Trivial criticisms aside, this is a valuable little manual which should soften many an initial plunge into scientific Russian.

NORMAN HENLEY
Johns Hopkins University and
Goucher College

Iceland Summer. Adventures of a bird painter. George Miksch Sutton. University of Oklahoma Press, Norman, 1961. xviii + 254 pp. Illus. $5.95.

Here is a delightful book about a 1958 adventure in Iceland, in company with the Olin Sewall Pettingills and such Icelandic naturalists as Finnur Gudmundsson and Árni Waag. George Miksch Sutton, professor of zoology at the University of Oklahoma, is an authority on boreal birds and a bird painter second to none. One aim of this voyage was to make a series of drawings showing the natal plumage of the many waterbirds and the juvenal plumage of the few song birds that breed in Iceland. Summer's end found over 30 drawings and two large boxes of bird skins prepared. The volume contains a magnificent colored frontispiece showing the gyrfalcon or *fálki*, Iceland's national bird, as well as superb portraits of the young of 17 other species. It is a pity that some of the latter were not also reproduced in color. Eight photographs (by Pettingill) help to depict the exotic country.

The text is equally charming, for Sutton writes with a gifted pen of this weird and beautiful country of contrasts—of hot springs and lava beds, of modern Reykjavík and remote Mývatn, of "the majesty of shining lakes and great white swans, of mountains rising abruptly from the meadowlands like huge cathedrals, of distant glaciers gleaming in the sun." He describes the celebration of Independence Day on 17 June, the day on which the Norse vikings first established their Althing (legislature) in the year 930. He tells

of a great black-backed gull attacked by a pair of whimbrels; of finding a redshank's nest deep in the grass. He recounts his thoughts while using a puffin chick as a model: "Though not quite shapeless, it looked as if evolution had been leading it headlong in that general direction for quite a while." Along the Fnjóská River a rare bird was discovered: "The gyrfalcon sat there with wings loosely folded, looking calmly, regally, off in the direction in which it had been headed, then toward us. Its perch was not dramatic in the least. The whole area was flat. The scene was not in any way 'composed'; there was nothing balanced about its few simple elements. But so beautiful was that single bird as it stood there, so majestic and powerful, that it needed no special lighting to give it appeal, no background of crag to make one hungry for a further look."

There is much else besides—of bird lore and general natural history, travel and adventure, good fellowship with true friends. Though quite unintentional, this sort of writing is the best diplomacy between diverse peoples today. Characteristically, Sutton has thoughtfully included a list of the common and technical names of all birds mentioned, with the Icelandic names of the 69 species found there—all but 14 of them waterbirds.

RICHARD H. MANVILLE
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The Export Economies. Their pattern of development in historical perspective. Jonathan V. Levin. Harvard University Press, Cambridge, Mass., 1960. 347 pp. $6.75.

The export economies are made up of a subset of underdeveloped countries whose advanced sectors (characterized by specialization of function and monetary exchange) are engaged primarily in the production of raw materials for export to advanced countries. These countries have received worldwide attention of late because many of them are currently going through substantial internal changes bordering on chaos, as in the case of the Congo, or they are actively engaged in wholesale expropriation of foreign owned enterprises within their control, as in Cuba. Levin has attempted to advance the understanding of their development, or lack of it, through the method of historical generalization. By investigating the economic history of some of these countries, Peru and Burma in particular, he hoped to distill a framework of behavior that could be considered as typical for this type of country.

The origin of the export trade usually sprang from the discovery of a natural resource crucial to the production of a product already greatly desired in the advanced countries. The areas where the resource was found, however, were backward in the extreme and were unable to supply the capital, the entrepreneurship, and in some cases the labor to transform the resource into an earning asset. These needed factors of production quite naturally came from abroad and had the effect of grafting an advanced foreign sector onto a primitive economy. Except for the payment of local taxes, the domestic economies gained very little from this development. The foreign factors of production either remitted their returns to the originating country or consumed imported products, but they did not stimulate production in the export economy. Within recent years, these countries have learned to capture a larger share of the export value through government regulation of the foreign factors or by direct government participation in the export industry, replacing the function and thereby earning the rewards previously going abroad.

Levin has no doubt adequately described the major factors in the development of the export trade in the countries he studied. I would disagree with him, however, as to the interpretation of why the indigenous labor of Peru did not respond to the opportunities of employment in the guano mines. He attributes this to the culturally determined unwillingness to leave their subsistence way of life. This evidence just as easily, or preferably, could be interpreted to mean that the wage incentives offered were too low to attract them. The major criticism that I find with Levin's work, however, is that it fails to explain why some other countries that have had all the pre-existing conditions of the countries described did in fact develop much differently in response to the export trade stimulus, namely the United States, Canada, and Australia. While it is true that many countries did follow the described pattern, the crucial question of why they did remains unanswered.

The book is quite readable, particularly the descriptive chapters 2 and 5. The historical generalizations in chapters 3 and 6 are also well done. The theoretical treatment, primarily in chapter 4, however, is disappointing, for it tends to be repetitious and Levin insists on fighting a battle over a nonexisting difference with traditional theory concerning the location of export industries.

LAWRENCE B. KRAUSE
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Trends in Government Financing. Morris A. Copeland. Princeton University Press, Princeton, N.J., 1961. xxvi + 210 pp. $5.

The National Bureau of Economic Research initiated, 10 years ago, a major study of the trends and prospects in capital formation and financing in the American economy. Copeland's volume is part of this project. It is, however, also of value in its own right. Copeland presents governmental—federal, state, and local—expenditures, receipts, and financial requirements covering the period from 1890 to 1950 for most of his data. Simon Kuznets points, in a preface, to the rising absolute and relative importance of government as a borrower. From 1900 to 1955 the liabilities of the government sector increased from 9 to 28 percent of total liabilities. In the same period the government's share in all tangible assets (excluding consumer durables) increased from 9 to 17 percent. On the basis of Copeland's study Kuznets concludes that "the role of governments is bound to become greater." I personally would limit this prediction to the nondefense sector of government because there is little basis for making long-term predictions about the relation of defense programs to the growth of the economy as a whole.

Copeland's study not only presents the facts but also demonstrates the need for clarification of basic concepts. For instance, the federal government publishes four different deficit or surplus computations. Copeland questions the political use of the term *deficit financing* and suggests that, in line with business practices, a distinction should be made between government borrowing to finance capital outlays, to meet emergencies, and to bridge temporary budget deficits. Because deficits or surpluses cannot be used as the criterion for good or bad policy, Copeland in-

troduces the qualitative terms *orderly* and *disorderly finance* and applies these terms quite successfully to a characterization of the financial policies of various local governments. While this is essentially a historical study, it offers many stimulating ideas for improvements in fiscal accounting, fiscal procedures, and fiscal policies.

GERHARD COLM
National Planning Association, Washington, D.C.

Fights, Games, and Debates. Anatol Rapoport. University of Michigan Press, Ann Arbor, 1960. 400 pp. $6.95.

Rapoport distinguishes three basic types of human conflict. The first, which he calls "fight," is dominated by a blind, destructive impuse to eliminate an opponent with whom one has nothing in common. In the second, opponents sharing a common set of rules seek to get the best of each other by applying appropriate strategies; for this kind of conflict, Rapoport uses the label "game." Finally, in a third variant of the basic theme of conflict, the objective is to reorient the opponent's thinking, to make him agree with *our* point of view which he rejects: this is what a "debate" is about.

In the first two parts of the book, dealing with "fights" and "games" respectively, Rapoport does not analyze fight-type and game-type conflicts but discusses two different theoretical approaches to the problem of human conflict: L. F. Richardson's mathematico-physical approach, and the theory of games. His treatment of both is a model of lucid exposition and judicious critique. The uninitiated will find in these parts of Rapoport's book an excellent introduction to Richardson's ideas and to the basic concepts of game theory, but those familiar with either will also learn much from his critical remarks.

The third part, devoted to "debates," represents an original venture into uncharted seas; necessarily so, since no formally elaborated theories comparable to Richardson's analysis of armament races and "deadly quarrels," or to the von Neumann-Morgenstern theory of games, exist in this field. Nor does Rapoport attempt to construct a rigorous axiomatic-deductive model of "debate" as a type of conflict; the subject indeed does not seem to lend itself well to this. Rapoport chooses the clinical approach instead. He distinguishes three types of debate, "brainwashing," "explaining the image away," and "removing the threats associated with alternative images." These correspond to Pavlovian conditioning, Freudian psychoanalysis, and "non-directive" psychotherapy (C. R. Rogers), respectively.

By distinguishing the "debate" as a separate category, Rapoport has made an important addition to the theory of conflict. It is clear that when our aim is to persuade or convert, we are neither acting "blindly" nor applying payoff-maximizing strategies. What Rapoport has to say about reorientation in general and Pavlovian, Freudian, and nondirective reorientation techniques in particular is also instructive and illuminating. Yet it seems to me regrettable that he put the analysis of "debate" in a framework of clinical psychology. Conflict is not of the essence of the clinical (experimental or therapeutic) situation; it may only be incidental to the clinical setting. The basic model of "debate" as a form of conflict is still the Socratic dialogue, and we need an elucidation of the logical and existential structure of this before we can fruitfully turn to the "debate" aspects of clinical psychology.

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Taylor's Encyclopedia of Gardening. Norman Taylor, Ed. Houghton Mifflin, Boston, Mass., ed. 4, 1961. xiv + 1329 pp. Illus. $15.

With limited review space available in journals and almost everyone pressed for time, new editions of standard reference works customarily receive no more than a citation. In this instance, however, something more should be said.

Those interested in gardening probably know that Norman Taylor's one-volume encyclopedia is an authoritative, helpful reference volume for both amateurs and professionals. This new edition is a substantial revision in that the entire text, before resetting, received the editor's scrutiny; the number of plant species and varieties was increased by about 10 percent (over 9000 are now included); new gardening techniques and products are described. For example, the articles on pests and insecticides have been simplified and updated, and throughout the text references to sprays and dusts are given by key numbers. In addition to the 500 line drawings, maps, and diagrams, there are now 48 handsome color plates—nearly 100 flower paintings by Eduardo Salgado and over 60 photographs of gardens. Doubtless the photographs are intended to encourage all readers to realize what gardens can be under optimum conditions. One hopes the price will not discourage those interested. After all, for *each* of the 15 dollars, there are over 1000 entries or about 66,667 words.

RAYMOND L. TAYLOR
American Association for the Advancement of Science

The Emerging States of French Equatorial Africa. Virginia Thompson and Richard Adloff. Stanford University Press, Stanford, Calif., 1960. 595 pp. $8.75.

Of all those arbitrary regions by which Europe affixed its claims to the African continent, one of the least known is French Equatorial Africa. The name, which has a precise and firm ring to it, suggests a cohesive entity, a people sharing a common tradition and shaping a common future.

Concerning things African, our vocabulary is frequently misleading; our labels conceal reality rather than illuminate it. French Equatorial Africa is a vast, inchoate area nearly one-third the size of the United States, strung out from the Congo River, well below the Equator, to the Tropic of Cancer. No more than 5 million Africans live there (about five to every square mile), representing a potpourri of tribes and having the most questionable of credentials for statehood. Yet such is the power of an idea in this age of the territorial state and independence-for-everyone that French Equatorial Africa, almost against its will, has suddenly been transformed into four sovereignties: Gabon, the Republic of the Congo (Brazzaville, not Leopoldville), the Central African Republic, and Chad.

It is high time we knew more of this area—the more so because it abuts the strife-torn Congo (the former Belgian territory). Thompson and Adloff's book, *The Emerging States of French Equatorial Africa*, is thus remarkably timely.

This is a ponderous book, perhaps unnecessarily so. Parts of it arouse as

much interest and enthusiasm as would a reading of the Manhattan Telephone Directory. But the book, like its predecessor and companion work, *French West Africa*, does not claim to be more than it is—not an interpretative study and not a political history (though the more important developments are well recorded), but a reference book. As such, it is the useful result of patient clipping, gathering, filing, and collating—a massive catalog of basic information; and thus an indispensable volume for scholars and analysts.

Part 1 is a review of institutions and development at the regional level, including all the principal categories in political, economic, and social analysis. Part 2, with some unavoidable duplication, arranges many of the same materials within the context of each of the former territories and summarizes the main developments in territorial politics. The volume includes a comprehensive bibliography.

ROBERT C. GOOD
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The Application of Quantitative Methods in Archaeology. Viking Fund Publications in Anthropology, No. 28. Robert F. Heizer and Sherburne F. Cook, Eds. Quadrangle Books, Chicago, Ill., 1960. x + 358 pp. Illus.

In 1959 the Wenner-Gren Foundation sponsored a 9-day meeting in Austria for the editors of this volume and ten other scholars selected by them. The results are published here in the form of 12 articles, along with discussions by the participants and a short list of "recommendations." The latter are primarily a plea for more rigorous analytical methodology.

Of the articles, six have their major emphasis on chemistry (of bone, soils, metals, and stone); four are primarily statistical (descriptions of artifacts and prehistoric populations); one is on chronology; and two are general (dealing with ceramics and habitation refuse). The unifying feature is that all the papers are concerned with the central problem of extracting a greater amount of information (and inference) from the scrappy residues of human activity collected from archeological sites. Since this is the major problem of archeological methodology, a broad-scale contribution such as this is most useful and most welcome.

Except for the over-all goal, however, there is little linkage between the individual contributions, for they treat several specialized techniques, each having separate laboratory methodologies, vocabularies, and research objectives. In this case uneveness is intensified because a small number of scholars have tackled an extremely broad subject, which here includes nearly all aspects of archeological collecting and analyzing. The expression of individual research interests in the papers is only partly offset by the group discussions at the end.

Despite the fact that all archeology is a sampling operation and an attempt to interpret the whole from the parts, archeologists have been remarkably resistant to the ideas of the few scholars who have stressed better control of the archeological sample and better use of sampling in interpretation. A principal contribution of this volume is its attention to sampling procedures, mentioned or referred to throughout the papers. A. C. Spaulding's paper on statistical description and comparison of artifact assemblages provides a good basis for more sophisticated analysis of archeological samples. As he points out, and as the rest of the papers affirm, archeology has now come to that stage of development which requires increased attention to quantitative methods.

CLEMENT W. MEIGHAN
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Museum Directory of the United States and Canada. Erwin O. Christensen, Ed. American Association of Museums (Smithsonian Institution), Washington, D.C., 1961. xx + 567 pp. $8.

The more than 4500 entries cover institutions in the entire museum field—art, history, science, and special fields, as well as college and university museums, botanical gardens, art centers and associations, and the like. Arrangement is alphabetical, by state and within each state by city. Alphabetical lists of institutions by categories are included. The main entry for each institution provides such information as the date founded, mailing address, name of the director, classification of major collections, publications, and visiting hours and fees.

Abstracts of New World Archaeology. vol. 1, 1959. Richard B. Woodbury, Ed. Society for American Archaeology, University of Utah Press, Salt Lake City, 1960. vi + 127 pp. Paper, $3.50.

The scope and aim of the abstracts is to include all published titles dealing expressly with New World archeology, including master's and doctor's theses. Presentations on theory, methods, and techniques will be included if they are pertinent to New World problems. The arrangement is geographical, with titles arranged alphabetically by author. An index of authors is included.

Visual Space Perception. William H. Ittelson. Springer, New York, 1960. 212 pp. $6.

This monograph ranges over the area of space perception, maintaining the general view that is characteristic of Adelbert Ames (to whom the book is dedicated). The attitude is said to be *functional* or *transactional*, and may be characterized by this direct quotation: "Perceiving is that part of the process of living by which each one of us from his own particular point of view creates for himself the world in which he has his life's experiences and through which he strives to gain his satisfactions" (page 19).

The monograph is divided into three parts. The first three chapters discuss the perceptual process and are, in a sense, an elaboration of the quotation given above. Part 2, the body of the monograph, covers the conventional visual space cues, but in a fresh manner that stems from Ames's fundamental question, "Given a perception, what is the related physical world?" (page 113). Part 3 brings out the role of attitudes and motivation in perception. It starts with a discussion of the possible resolutions of conflicting perceptual cues, such as size and convergence, and ends with a discussion of experiments with a stereoscope in which two faces of varying degrees of familiarity and emotional value are found to exhibit characteristic degrees of fusion and rivalry.

In the introduction Ittelson states that several sections of the present book have been adapted from his previous publications made with associates. This may account for the fact that many of his most significant statements seem

overly terse, abstract, and dogmatic. He also has an annoying habit of citing people without keying the citation to the list of references at the end of the book. The failure to document experiments outside the "in-group" is especially serious in view of Ittelson's arbitrary summaries of these experiments. He spends more space on the details of the experiments of his own group, but warns that the reader should examine the original experiments. I second this warning.

Incidentally, a rough count of his 144 cited references shows that more than a quarter of them belong to what may loosely be called the Ames group and that Ittelson was author or coauthor of a dozen of them. So we may be led to accept this as a definitive summary of the neofunctional or transactional approach to perception. However, we may question whether it does full justice to the important insights that this approach has achieved.

HAROLD SCHLOSBERG
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College Textbooks. Compiled by Jane Clapp. Scarecrow Press, New York, 1960. 1096 pp. $25.

A classified listing of 16,000 textbooks used in 60 colleges, with author-title and subject indexes. The books are classified under three major headings: humanities, sciences, professional and technical fields.

Biological Sciences

The Wellsprings of Life. Isaac Asimov. Abelard-Schuman, New York, 1961. 238 pp. Illus. $3.75.

The Long Road to Man. Robert L. Lehrman. Basic Books, New York, 1961. 200 pp. Illus. $3.95.

These two books are both intended to convey present knowledge of organic evolution to laymen, but otherwise they are extraordinarily different. Lehrman's book is simple both in language and in thought, at times to the point of naiveté, and it is pitched at a low scholastic level. It is published in a series advertised by the publisher as intended "for teen-agers (and grown-up readers as well)." Asimov's book, although equally comprehensible, is a sophisticated, intelligently adult work. Lehrman tells a straightforward story of the long phylogeny of man. Asimov slides quickly over that subject and is almost entirely intent on underlying functions, for the most part in terms of the research that has revealed them. The juxtaposition of the two books emphasizes the great complexity of their common subject, for there is little duplication between them and both together still omit large and important parts of the subject.

After an introductory chapter, Lehrman devotes the next 10 of his 14 chapters to a summary of the fossil record of the groups in or near the human ancestry. The 12th chapter deals with ways of supplementing the fossil record by evolutionary interpretation of recent animals, especially of their embryology, and then briefly discusses the origin of the chordates. Chapter 13 reviews the whole story from the origin of life to *Homo sapiens*, by a combined approach. The themes of the last chapter are that man's coming was an evolutionary revolution, that our biological evolution ends here, and that purely cultural evolution now takes over and alone determines our future.

This storybook restatement of the evolutionary history of man is welcome. Lehrman's book is also generally up-to-date, and it is admirable in some other respects, for example, in much of its handling of sequence and local variation in fossil hominids. It is usually accurate, but by no means wholly so. I noted more than 40 errors, by which I do not mean statements open to misunderstanding, exception, or dispute (which are also rather numerous) but such downright mistakes as the remark that reptiles have their skulls supported on two bony projections (occipital condyles) and amphibians on one, or that *Cynognathus* had flat-topped molars—not world-shaking errors, but they do count up.

It seems to be almost inevitable in popularization of this sort that a few pseudo-Lamarckian and anthropomorphic or teleological phrases slip in: the environment converts a creature into something else; an animal retains such of its heritage as it finds useful. Lehrman's remarks on evolutionary processes do properly stress the interplay of mutation and natural selection, but he misrepresents natural selection as tending always to eliminate variation and leading inevitably to overspecialization. His belief that no natural selection or indeed no biological evolution is occurring in modern man is shared by few biologists and will be considered a dangerous doctrine by many of them. The book would have been more satisfactory if it either had stuck entirely to its main, descriptively historical theme or had given a more adequate and more insightful interpretation of the history.

The book by Asimov, quite different in approach and intellectual content, has also different merits and shortcomings. Asimov is one of our natural wonders and national resources. By profession he is listed as a university professor of chemistry. It is presumably in his spare time that he turns out three or four books and one or two dozen articles per year. He was once one of the prolific pioneers of modern, literate science fiction. He has gone on to become probably the most prolific and certainly among the best of the literate popularizers of science fact and theory. He is most at home in the physical sciences, but his works now embrace practically all fields of natural science and some others.

In the present book, with important omissions to be noted later, Asimov briefly spans much of the field of evolutionary biology. Here, as usual, his style is clear without sacrifice of solid content. He is continuously interesting and occasionally amusing without condescension or jocular superficiality.

The organization of the book is by decreasing levels of complexity and integration, starting with species and higher aggregations of populations of whole organisms and ending at the molecular level and, in particular, with deoxyribonucleic acid or DNA, presented as the ultimate physiochemical basis of evolution. The approach at each level is in the main historical, in terms of the successive discoveries of about 100 biologists and physical scientists. An appendix telegraphically lists workers and events in the history of evolutionary biology chronologically from 1630 ("Archbishop James Ussher calculates date of Creation as 4004 B.C.") to 1960 ("O. Struve plans radio telescope survey to detect life in other stellar systems").

The bulk of the book is in three parts of five chapters each (a symmetry achieved at some sacrifice of logic): Life and the Species; Life and the Cell; Life and the Molecule. A final part, headed simply "Life," deals in one chapter with the origin of life, on earth and speculatively elsewhere. The scope and trend of the story may be suggested by a partial list of topics and authorities discussed in sequence. Spontaneous generation: Aristotle, Redi, van Leeuwen-

hoek, Schwann, Pasteur. Classification: the Bible, Aristotle, Ray, Linnaeus. Basic concepts of uniformitarianism and evolution: the Bible, Hutton, Bonnet, Smith, Cuvier, Lyell, Lamarck, Darwin, Spencer, Mendel, De Vries. Two chapters then review, in barest essentials and without illustrations, the historical sequences from "one-celled creatures" to, among others, man. Consideration of organisms as such practically stops there.

Discussion continues around the level of cells and formal genetics: van Leeuwenhoek, von Baer, Haeckel, Flemming, and so (with some others more briefly mentioned) back to Mendel, who then has a chapter to himself, and to mutations (Muller, Pauling). The third part is entirely biochemical and runs from the first organic synthesis (Wöhler) through carbohydrates (Kirchoff, Bernard), lipids (Chevreul), proteins (Braconnot, von Liebig), structural formulae (Kekule, Fischer), and enzymes (Réaumur, Pasteur, Buchner, Sumner and Northrop, Beadle) to viruses and nucleic acids (Levene, Stanley, Avery, Fraenkel-Conrat, Pollard, Crick and Watson, Gamow, Hoagland, Allfrey and Mirsky, among others). The chapter on the origin of life and life on other worlds then closes the book.

That is an exceedingly long and complex list of subjects to treat in few more than 200 uncrowded pages. That it is treated for the intelligent layman so interestingly and clearly is an admirable *tour de force*. It would be unreasonable to expect that there should be no oversimplifications, no faulty generalizations, and no errors. Suffice it therefore to say that there are a few but that they do not detract seriously from the overall view that will be gained by a nonspecialized reader.

Cleavage in Modern Biology

By one criterion, legitimate criticism of Asimov's book ends there. What more I have to say relates less to the author's inclusions and accomplishments than to his omissions and limitations. Yet these further remarks are pertinent and have an importance broader than this particular occasion, because they relate to an unfortunate cleavage in modern biology brought about by an attitude among some (not all) biochemists and molecular biologists. It is largely a matter of the definition of goals in biology. To me and to other nonchemical biologists, it seems obvious that the most important goal of biology is to understand organisms: their structure, their functions, their interrelationships (of all kinds), and their history. It is no less obvious that biochemistry and molecular biology are absolutely essential for further progress toward that goal. To the extent, however, that biochemists and molecular biologists approach their materials from a physicochemical point of view, not clearly related to organisms as such, their studies lack truly biological significance and, however fashionable they may be, can hardly be in the ultimate mainstream of the science of *biology*.

That failing becomes vital when, as in Asimov's present book, the subject is organic evolution, which certainly has its meaningful focus at the level of populations of integrated organisms. The fact is that as Asimov follows his sequence *away* from that level his hand becomes surer and his information more adequate and modern. On systematics and the over-all phenomena of evolution his exposition is dated by from 75 to more than 100 years. His concept of the basic evolutionary process is that of some 50 years ago. (Interestingly enough, this is also true of current science fiction.) Apart from one or two minor points and the strictly chemical aspects, the rest of his genetics is from 30 to 40 years out of date.

It is in one way somewhat refreshing but in another appalling to read a general book on organic evolution that includes no hint of the work and (although written so largely in terms of personalities) no mention of the name of a single one of those to whom modern knowledge of the evolution of *organisms* is due. The Wright in the index is not Sewall but Seth, a farmer immortalized because in 1791 his flock produced a mutant lamb. The Huxley is Thomas Henry, obiit 1895, not Sir Julian, so fortunately still with us. W. J. Bryan (of *Inherit the Wind*) is there and so are W. S. Gilbert (of *The Mikado*) and W. Shakespeare (of *Antony and Cleopatra*), but W. C. Allee is not, nor, to mention only a few others, among many, are either C. D. or P. J. Darlington, Dobzhansky, Fisher, Haldane, Hardy, Lerner, Mayr, Rensch, Schmalhausen, or Waddington.

Only when Asimov finally reaches DNA does his book really reflect the state of knowledge in 1960. Here was the opportunity, one might say the obligation, to circle back, to bring the rest of the story up to date, and to tie all together in the meaningful terms of what is ultimately to be explained. The author's final words are, "A nucleic acid molecule came accidentally into being that could somehow bring about the existence of another like itself—And from that all else would follow!" But he has not really shown at the level of organisms, of *ourselves*, how or why it followed.

The disappointment is not exactly that of seeing Hamlet played without the Prince of Denmark. We can grant Asimov's casting of DNA as the Hamlet of his drama. The stage is set most elaborately. The supporting cast is brought on, one by one, and an intricate plot is developed for eventual resolution by the hero. All builds up in a crescendo to the entrance of DNA —Hamlet. Finally the hero does enter, and—

Just stands there!

G. G. SIMPSON
Museum of Comparative Zoology, Harvard University

Late-Pleistocene Environments of North Pacific North America. An elaboration of late-glacial and postglacial climate, physiographic, and biotic changes. Special Publication No. 35. Calvin J. Heusser. American Geographical Society, New York, 1960. 308 pp. Illus. Paper, $4; cloth, $6.

This is a comprehensive study of late-Quaternary pollen sequences and related chronologies for the Pacific coast from California to south-central Alaska and Kodiak Island. The province is that of the Pacific Coast Conifer Forest, a natural region possessing unifying features but extending over more than 20 degrees of latitude. The fossil pollen evidence upon which the study is based is original, and most of it is published here for the first time. Carefully integrated with the accumulated evidence from geology, glaciology, and phytogeography, these palynological data yield information about the history of the Pacific Coast Conifer Forest that is of fundamental importance to an understanding of late-Pleistocene events in northwestern North America.

Calvin Heusser began this study in 1950 in connection with work on the late-Pleistocene history of the Juneau Ice Field, Alaska. By 1958 he had extended these palynological investigations westward to Cook Inlet and Kodiak Island and southward to California. His aim was to reconstruct the post-ice age environments and history of the

Pacific Coast Conifer Forest throughout its range. Altogether 114 sections in peat and other sedimentary deposits were studied; they form the basis for this work. Seventy-eight sections and their pollen profiles are presented. Eleven sections were reported previously; they were restudied in light of the total evidence, and the results were included. Henry P. Hansen, the pioneer in Pacific Northwest palynological investigations and the professor with whom the author studied, wrote the foreword, which includes an appreciation of Heusser's accomplishments.

The book presents a highly satisfactory coverage of the manifold aspects of events during the last 13,000 years along this coast. The geographic setting is well described, with sections on late-Pleistocene geomorphology, modern climate and recent climatic trends, and characteristics of the modern vegetation within the seven districts distinguished. A succinct review of Quaternary palynology's principles, methods, and sources of error introduces the main section of the book, a detailed and critical description of the peat and pollen stratigraphy of the sections, grouped according to the five regions within which the sequences are similar.

The more significant conclusions from these individual investigations are summed up with a discussion of late-Pleistocene vegetation in the total area, the environments inferred, and correspondence of the vegetation history with chronologies of other events, especially sea level changes relative to the land. A final chapter examines major biogeographical problems of the area. An appendix contains detailed information about sampling sites and an account of the geology and landforms in the various physiographic divisions. Although its definitions are in some instances too terse, the glossary provided will be welcomed by the reader unfamiliar with the specialized terms that Heusser could not avoid using.

The author's interpretations of his pollen profiles are consistent with climatic interpretations of available vegetation chronologies for areas as distant as Japan, New Zealand, Fuego Patagonia, and the British Isles. Thus further support is given to our growing confidence in the synchrony of worldwide climatic fluctuations that resulted in a warming trend during the retreat of the last continental ice sheets (Late Glacial and Early Postglacial), a time when warmth exceeded that of the present (the Hypsithermal Interval), and a temperature regression leading to the present (Late Postglacial). However, the Hypsithermal Interval is shown here to be noticeably shorter at the high latitudes. The Late Postglacial began about 2500 years ago in California, but 4000 years ago on Kodiak Island.

Interpretations of the pollen profiles are brought together with evidence from glacial geology and physiographic studies to lend new support to two of Hultén's postulated ice-free refugia, namely those in south-central Alaska and in the coastal strip from southeastern Alaska to Washington and Oregon. This new palynological evidence is also used in an attempt to determine the outlines of late-Pleistocene plant migrations. Although definitive solutions will require additional evidence, good cases are made, for example, for the migration of lodgepole pine westward as far as the Yakutat flats during the Postglacial and for two migrations of Interior Forest elements onto the Kenai Peninsula from unglaciated central Alaska, the more recent of which probably occurred during the last 4000 years.

This very readable and well-designed book will be, for some years, a unique authoritative source for information on late-Pleistocene vegetation of the Pacific Coast of northwestern North America and its chronological relations. The book is indispensable to one concerned with biogeography and Quaternary history in that part of the continent.

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Animal Sounds and Communication. W. E. Lanyon and W. N. Tavolga, Eds. American Institute of Biological Sciences, Washington, D.C., 1961. 443 pp. Illus. Demonstration record (33⅓ rpm): Sounds of insects, fishes, frogs, and birds. 2 sides. Members, $8.50; others, $9.50.

Most of the papers in this book were presented at a symposium of the Ecological Society of America, organized by J. T. Emlen, Jr., at the annual meeting of the American Institute of Biological Sciences in 1958. It would be unfortunate to give the impression that this represents even a reasonably complete survey of the vigorous field of bioacoustics. There are no papers dealing other than superficially with sounds produced by mammals, by insects other than Orthoptera and cicadas, or by other invertebrates, and the coverage of receptive mechanisms involved in acoustical behavior is extremely scanty. Within its limited framework, the book offers interesting and challenging essays on the selected subjects.

The papers vary widely in length and coverage. Thus, an article by C. M. Bogert on sounds and behavior of amphibians and reptiles is 183 pages long, almost half the book; it is a broadly critical review with a detailed presentation of Bogert's own work. On the other hand, the article by D. J. Borror on the analysis of animal sounds is only 11 pages long, most of them devoted to analyses of bird songs; it deals with only one type of sound spectrograph, omitting discussion of even so basic a tool for acoustic analysis as the oscillograph.

The article by P. P. Kellogg on tape recorders and recording, which, with Bogert's, was added after the symposium, is a workmanlike discussion. Unfortunately some of the warnings given by Kellogg were not heeded by some of those who prepared samples for the demonstration record. The record is a novel and useful adjunct, but disappointingly marred by print-through echoes and by distortion caused by overloading in the original recordings in many of the sections.

R. D. Alexander, in a paper on acoustical communication in Orthoptera and cicadas, combines a brief review of the literature with interesting personal observations and a number of sweeping generalizations that should take a few life-times to test. W. N. Tavolga presents challenging observations on underwater communication in fishes and emphasizes the need for more research.

The ontogeny of vocalization in birds is considered by W. E. Lanyon in the light of recent studies in Europe and the United States. The place of bird songs in mate selection is discussed by P. Marler, particularly the many problems still to be investigated.

The last two essays in the book, as well as the brief introduction by J. T. Emlen, Jr., are more general in approach. N. E. Collias attempts an ecological and functional classification of animal sounds. This "outline for continued research," as he aptly calls it, recognizes sounds as related to food, enemies, sexual behavior, parent-young relationships, and group movements.

C. F. Hockett concludes the book with a stimulating paper on logical considerations in the study of animal communication, in which he compares animal signals with human signals in message-carrying capacity.

The fact that another book of at least this size could be produced with papers on acoustical communication in mammals, insects, and other invertebrates, on instrumentation and analysis, and on auditory mechanisms shows that the field of bioacoustics is lively. The call for more research made in most of the papers shows that the field should grow even more lively each year.

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Microbiology. Historical contributions from 1776 to 1908. Raymond N. Doetsch. Rutgers University Press, New Brunswick, N.J., 1960. xiv + 233 pp. Illus. $5.

If we agree with the author's book plan, this little volume rates as a valuable contribution. It provides material for general courses in microbiology and supplements study of a poorly documented branch of that subject. The period covered is from 1776 to 1908, although only one of 15 scientists considered was born before 1776 and nine lived beyond 1908. The papers cited are restricted to those revealing historical antecedents of microbiology before it was arbitrarily compartmentalized.

The text begins with the origin of microbes, considers their role in fermentation, moves on to laboratory methods, and discusses classification and, lastly, biological activity. Each chapter begins with a *brief* historical introduction by the editor; this is followed with an article by the scientist concerned.

Pasteur's report on lactic fermentation requires three pages, whereas 26 are given to Lister's paper on lactic fermentation and its bearing on pathology. Schloesing's two pages on nitrification by organized ferments is the briefest paper presented. Beijerinck's discussion of *Spirillum desulfuricans* requires 23 pages, and Cohn's lecture, "Bacteria, the smallest of living organisms," 22. Koch rates two chapters, both on methods, and two present Winogradsky's work on sulfur bacteria and nitrifying organisms.

Theobald Smith is omitted, but Erwin Smith's paper, "Are there bacterial diseases of plants," is a worthy selection. Orla-Jensen's 16-page paper, "Main lines of bacterial systems," concludes the book and should particularly interest taxonomists.

Eighteen full-page plates portray each of the 15 scientists, show "Cohn's first plate illustrating bacteria," a page from Beijerinck's notebook, and an illustration of apparatus and organisms from the Lister paper. There are also a number of smaller figures and a few tables. There is no index or bibliography, although each chapter ends with a short list of suggestions for further reading.

Though the book is intended for students of microbiology, it should be of considerable interest to all in the field. The less mature may need guidance in reading it, if they are to preserve a proper balance respecting the over-all place of the scientists discussed; the more mature will find their background greatly improved by its perusal.

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Mushrooms of the Great Smokies. A field guide to some mushrooms and their relatives. L. R. Hesler. University of Tennessee Press, Knoxville, 1960. xii + 289 pp. Illus. $5.50.

During his career of nearly 40 years at the University of Tennessee, Hesler has devoted as much time as official duties have permitted to the critical study of the fungi of Great Smoky Mountains National Park, an area noted for the number and diversity of the species present. The results of his studies have appeared heretofore as technical papers, but in the present volume he offers, in a more popular form, an account of some of the interesting species occurring in the park.

After a brief introduction covering such topics as edible and poisonous mushrooms, fairy rings, mycorrhizal associations, luminescent fungi, and even the hallucinogenic forms, nearly 200 species selected from the more than 2000 known to occur in the region are described, briefly but accurately, and illustrated. Each description involves macroscopic characters only, with particular reference to shape, size, color, odor, and taste and with comment on its edibility, if known. The illustrations (in black and white) are by the author, and they are uniformly excellent. The book is well designed, the presswork is excellent, and the waterproof binding will protect copies used in field work. The book will be useful not only to "inquiring amateurs," as the author suggests, but to anyone concerned with the larger fungi of the park or surrounding areas.

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Arizona Flora. Thomas H. Kearney, Robert H. Peebles, and collaborators. Second edition with supplement by John Thomas Howell, Elizabeth McClintock, and collaborators. University of California Press, Berkeley, 1960. viii + 1085 pp. Illus. $12.50.

This excellent standard work on the flora of Arizona is reprinted, and it is brought up to date through a 47-page supplement composed of additions and changes in the treatment of species and of references to literature which appeared between 1951 and 1960. The revision is based upon extensive notes and published papers by Thomas H. Kearney, upon research and review of the literature by the revising authors, and upon revision of individual genera and families by the specialists who contributed portions of the text for the first edition.

The death of Thomas Kearney and Robert Peebles, both in 1956, marked the passing of a remarkable research team, for these men collaborated not only in taxonomic work but also in breeding cotton, a field in which their contributions are among the most outstanding of all time.

The main text stands essentially as it was printed in 1951. Even without the supplement this is one of the great treatments of a regional flora. Unfortunately, because of the cost of making new plates, one change of form was not possible in this revision. In the next edition the cumbersome and difficult-to-use nonindented keys should be changed to indented keys. This, of course, is a mere matter of mechanical rearrangement, but it is an item of first importance.

The more extensive revisions appear in *Selaginella, Bromus, Yucca, Quercus, Chenopodium, Argemone, Lupinus, Astragalus, Lathyrus, Argythamnia, Fraxinus, Gentiana, Asclepias, Gilia, Lithospermum, Valeriana, Hymenopappus,* and *Cirsium.*

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Kingdom of the Octopus. The life history of the Cephalopoda. Frank W. Lane. Sheridan House, New York, 1960. 300 pp. Illus. $7.50.

A Biography of the Sea. The story of the world ocean, its animal and plant population, and its influence on human history. Richard Carrington. Basic Books, New York, 1960. xvi + 268 pp. Illus. $5.

Under the Sea. Maurice Burton. Franklin Watts, New York, 1960. 256 pp. Illus. $4.95.

These three books have one thing in common; their authors desire that nonscientists read and enjoy them. Lane attempted a more difficult task by writing for scientists and nonscientists alike.

Kingdom of the Octopus must rate as a classic. Lane's craftsmanship as a writer and his years of hard work on subject matter have indeed led to a book valuable to scientists; at the same time, the volume should make splendid reading for the interested layman and is to be recommended for content and style.

A Biography of the Sea is also worthwhile reading. It tells of the geological development of seas and of the life in them and includes chapters on man's exploration of things marine. I question whether Carrington has produced a book which can be handled by nontechnical laymen other than those willing to read by dictionary. Carrington could be a paleontologist. He shows sympathy for and interest in the wondrous evolutionary changes in animals and in the earth itself which are far more sensitive than trained paleontologists and geologists are likely to show. The field is in need of such a competent writer; let us encourage him to continue writing on scientific subjects.

Under the Sea unfortunately falls short of being a good popular book on marine science. Hypothesis is not clearly labeled as such, and the parade of short descriptions of animals makes one dizzy.

Two of these writers have attempted a difficult thing: to synthesize not just a field but a whole area of science into a single book which does not misrepresent fact. After reading all three books, I wonder if this is really possible. Carrington could not bear to write of oceans or environment without first discussing some of the elementary physics of heat transfer. His desire that readers understand the importance of this to oceanography is commendable. He also realized the importance of the entire field of historical geology in tracing the history or biography of the sea, but one cannot do justice to paleontology in a chapter or two. The result is crowded, delightful reading for a trained person but rough going for those without specialized vocabularies.

Lane, on the other hand, reduces the scope of his book to squids, cuttlefish, and octopuses, and in so doing he gets deeply involved in many basic marine problems, such as bioluminescence, coloration, and environmental relationships. One gets the feeling that the 900-odd titles in his bibliography probably cover the entire world reading list on these animals. The illustrations show the same selectivity, for they have been chosen as the finest from many pictures. The photographs alone are worth having in one's library.

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The Beetles of the United States (A manual for identification). Section 1. Ross H. Arnett, Jr. Catholic University of America Press, Washington, D.C., 1960. xi + 210 pp. Illus. $3.50. (To be completed in about 2 years, in approximately eight sections, at a subscription price of $25. Binder available for $3.95.)

The ecologist or general biologist should find this manual very useful for the identification of the beetles he encounters in his field studies. Amateur and professional entomologists will find it indispensable. It is the avowed successor to J. Chester Bradley's mimeographed "Manual of the genera of beetles of America, north of Mexico" (1930), a work somewhat out of date and almost unavailable.

Section 1 comprises a general introduction, brief sketch of beetle anatomy, a bibliography of general works, a key to families of the world, and nine family fascicles (for the families Cupedidae, Rhysodidae, Cicindelidae, Carabidae, Amphizoidae, Haliplidae, Dytiscidae, Noteridae and Gyrinidae). The largest family covered is the Carabidae or ground beetles; the treatment of this family is a major generic revision for the Nearctic region, prepared by George E. Ball of the University of Alberta.

The plan for the whole manual calls for a separate fascicle for each of the beetle families found in the area covered —the United States from Canada to Mexico. The families treated in section 1 are brought out in their generally accepted sequence. For each family there is a short synonymy of family names; a common name; description of general features in adult and larval stages; a superb habitus drawing of one species and often some drawings of details; a brief account of the ecology, general distribution, and number of species in the world; and an appraisal of the state of the taxonomy. This introduction is followed by a key to genera and higher categories, a listing by these categories which includes the number of described species and the range for each genus as it is represented in the United States and sometimes an account of the ecology, and a family bibliography. Some of the fascicles have been checked by specialists in the families concerned, and the Carabidae fascicle was completely written by a specialist. The first nine fascicles have been handled in a very scholarly manner.

When completed, this manual will be a valuable aid to beetle identification, at least to the level of the genus. In secondary schools it should become as much a part of the library as a manual of botany, so that the budding naturalist can get some specific information about any beetle he may encounter, for beetles make up at least a fifth of all organisims in most areas. Its place in college and departmental libraries is assured.

I am sure that the publishers regret the price that they must charge for this work, which should get into the hands of anyone interested in insects. We can only hope that the price will enable them to keep the manual in print for a reasonable number of years. The method of publication, on pages 6 by 9

inches, punched for a 3-ring binder, will certainly be hailed by the users and bemoaned by librarians. A more permanent binding than this loose-leaf notebook will have to be used on library copies, which are sure to see heavy use.

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Hackfruchtkrankheiten und Nematodenforschung. Festschrift anlässlich der Einweihung des Neubaues für das Institut für Hackfruchtkrankheiten und Nematodenforschung der Biologischen Bundesanstalt für Land- und Forstwirtschaft in Münster (Westf.). (*Mitteilungen aus der Biologischen Bundesanstalt für Land- und Forstwirtschaft Berlin-Dahlem*, vol. 99). Parey, Berlin, 1960. 119 pp. DM. 13.50.

The dedication of a new buliding, like the baptism of a child, is an act of faith. It is an assertion that the purposes for which the building was erected will remain valid for years to come. It is also, like the religious ceremony, a celebration of the maturity of the parents. This *Festschrift* marks the dedication of a new building devoted to research on the diseases of truck farm crops and on the nematodes that are parasitic on plants. Three technical papers on nematodes and four on viruses report current research at the institute and are of interest to specialists in the field. Of more general interest is the brief history of the institute and the excellent review of phytonemotology in Germany.

Although soil-inhabiting nematodes have been known for many years, they have never been a part of the mainstream of biological research. Our scrutiny of the various groups of organisms about us has been strongly influenced by their size, brilliant coloration, or direct importance to man, either medically or agriculturally. The recent upsurge of work on the microscopic nematodes inhabiting soil and plants is a reminder that we still have an imperfect view of life on our planet. During the last two decades the number of workers in this branch of nematology has increased at least tenfold, but the field is still very small. The first German station for work in phytopathology was erected during the late 19th century to combat calamitous losses caused by nematodes in sugar beet crops. Little progress in understanding other plant diseases caused by nematodes was made until quite recently, when nematodes have been implicated in many plant diseases, both as direct pathological agents and as parts of complexes. The dedication of an institute having adequate space and facilities for research in this area represents an important step forward for German agriculture. We are happy to welcome this newest addition to research on nematology and look forward to the fulfillment of the promise of its new building.

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Physical Sciences and Engineering

Crystal Structure Analysis. Martin J. Buerger. Wiley, New York, 1960. 668 pp. Illus. $18.50.

Imperfections in Crystals. H. G. Van Bueren. North-Holland, Amsterdam; Interscience, New York, 1960. 676 pp. Illus. $16.75.

Here are two valuable additions to the summarizing literature of solid-state science. The two have different goals, but both will remain prominent sources of knowledge for a decade or more.

Martin Buerger, in addition to being an outstanding research investigator, is a pre-eminent teacher. His book is intended primarily to help the serious advanced student who wants to master the field of x-ray crystallography along his way. The volume grew out of nearly 15 years of experience in the lecture room and the teaching laboratory, and it represents a basic summary of generally valuable material. His recent book, *Vector Space and Its Application in Crystal Structure Investigation* (Wiley, New York, 1959) is in the nature of a supplement on more advanced topics.

Crystal Structure Analysis deals thoroughly and logically in a step-by-step way with each of the topics that the would-be professional crystallographer must have at his fingertips. The diffraction relations are treated both qualitatively and quantitatively. The ways for selecting and preparing specimens are described in detail. The various methods for measuring intensity and the factors that affect intensities are discussed thoroughly. The unit cell and its variations are described. The concept of the structure factor is developed, and, for illustrative purposes, the principles are applied to several simple crystal structures. The topics of Fourier synthesis, reciprocal lattice theory, and all of the implications of symmetry are treated in a direct and pragmatic way. The methods of determining the phases of Fourier coefficients, in both general and special cases, are reviewed; the results are applied to some of the standard problems.

The text is very clearly and painstakingly written, and it is profusely illustrated with clear figures and excellent tables. I do not believe the book has a rival, at least in the English language. It should become an indispensable aid in teaching basic x-ray crystallography wherever English is used.

Van Bueren's book on crystal imperfections represents the first really comprehensive attempt to discuss in detail the broad sweep of research on imperfections in crystals, particularly of the research since 1945. Other books, more specialized or otherwise, which are more critically written, may come in the future, but this book has all of the principal ideas laid forth in a very direct and readable manner. The author, a member of the Philips Research Laboratories (Eindhoven, Netherlands), obviously had the advantage of discussing the text with a substantial number of informed colleagues.

The book begins with a general account of the perfect crystal and the types of imperfections which it can sustain. The importance of temperature and the accidents of growth and chemical composition are described.

The second part deals with imperfections in metals, giving an account of the causes and effects of plastic deformation and of the importance of foreign atoms, diffusion, and the myriad of matters of interest in modern metal physics. It should be mentioned that almost two-thirds of the volume is concerned with the properties of metals. Part 3 deals with the salts and monatomic semiconductors, such as silicon and germanium. It uses the background of concepts introduced in discussing the metals, emphasizing the characteristic similarities and differences between metals and nonmetals.

It should be emphasized that the book is essentially qualitative or semiquantitative in nature in the sense that much of the material is treated in a descriptive way. On the other hand, the most basic equations which are important in the theory of imperfections are presented and discussed in a readable way. If the book can be said to have an imperfection of its own, it is that the writer remains somewhat impartial in discussing the various views held by his colleagues throughout the world. On the whole, he states their cases, even when their viewpoints differ; he is willing to remain a somewhat unimpassioned chronicler of the facts as he sees them.

The book should do a great deal to make the subject matter available in an understandable way to those outside the family of solid-state physicists and chemists responsible for its development. For this reason, it should have very general value.

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The Theory of Space, Time, and Gravitation. Vladimir Fock. Translated from the Russian by N. Kemmer. Pergamon, New York, 1959 (Moscow, 1955). xviii + 411 pp. $15.

Although his book is intended to be a text, academician Vladimir Fock's approach to Einstein's theory of gravitation is sufficiently unconventional that the translation affords English and American readers a welcome opportunity to examine the ideas as presented by the author himself. Besides, the book is written so clearly and lucidly that it is a pleasure to read, though perhaps, in view of the price, not a pleasure to buy.

To the extent that Fock's book covers standard topics in special and in general relativity, there is little need for a detailed review. Very naturally, the contributions by Soviet physicists and mathematicians receive more attention than works written in the West; there is, however, no tendency to claim credit that is not well documented.

In his introduction, Fock presents his views about the philosophical aspects of the special and the general theory of relativity. Whereas he agrees that the special theory of relativity is indeed "relativistic"—the geometry of the space-time manifold is homogeneous and isotropic—he asserts that in Einstein's theory of gravitation (normally called the general theory of relativity) the homogeneity of space-time is reduced; accordingly, Fock calls the latter theory the theory of gravitation. To me, Fock's attitude appears questionable. The introduction of a Riemannian metric into a co-ordinated space-time manifold introduces no more anisotropy and inhomogeneity into that space than does, for example, the introduction of an electromagnetic field into a Minkowski space. Whereas the basic geometry of the special theory of relativity contains a flat metric, which itself is not a physical field and does not obey dynamical laws, the metric tensor of the general theory of relativity is a physical field, determined by the dynamical laws of physics, and to be viewed as part of a particular physical situation. The properties of space-time that are not subject to dynamical laws but form, as it were, the absolute substratum, or stage, on which the physical events take place, are purely topological. Einstein conceded the possibility that in the future even these remaining "given" properties might be eliminated or reduced.

The significance of the requirement of general covariance, that is to say the equivalence of all curvilinear co-ordinate systems, appears to me to lie in the "physicalization" of the metric. Admittedly, the physical principle of equivalence is frequently overstated or misstated; its experimental foundations are somewhat slender and at present subject to critical re-examination.

Quite properly, Fock calls attention to the importance of boundary conditions in addition to the local differential equations governing the dynamics of a physical field. He suggests that in all physically interesting cases the metric turns flat at very large distances, a fact that does not follow from the field equations (the local laws) alone. He appears to conclude that Einstein's theory of gravitation should be considered, from the global point of view, as another Lorentz-covariant field theory, and he has worked out a scheme that reduces, by means of co-ordinate conditions, the invariance group of the theory to the Lorentz group. Very recent work appears to indicate that this point of view cannot be maintained as a matter of mathematical fact, though it is perhaps more cautious to consider the question as open, for the time being. That actual gravitational systems occurring in nature look as if they satisfied the requirement of Lorentz invariance only might well be considered a disturbing fact that tends to throw some doubt on the eventual validity of the invariance postulates of the general theory of relativity. But then, the properties of the cosmos at large even cast some doubt on the validity of the Lorentz group as well. Very obviously, the fundamental properties of space and time, as they relate to very large, and to very small, dimensions, are not yet entirely understood.

All in all, Fock's book, written by one of the great living theoretical physicists, is a thought-provoking work that will be of interest both to the specialist and to the general scientist interested in the philosophical foundations of contemporary theoretical physics.

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Cosmic Radio Waves. I. S. Shklovsky. Translated by Richard B. Rodman and Carlos M. Varsavsky. Harvard University Press, Cambridge, Mass., 1960. xvi + 444 pp. Illus. $12.50.

It is Shklovsky's position that radio astronomy cannot be discussed apart from optical astronomy and the subject of cosmic rays. In texts on radio astronomy, this unified viewpoint is quite unusual but very important. It makes the discussions much more valuable, and leaves the reader with a well-integrated picture of the processes in our galaxy which lead to the emission of radio waves, cosmic rays, and light.

After a concise technical introduction to the special problems of the detectors used in radio astronomy—receivers and antennas—Shklovsky reviews the basic observational results on galactic radio waves. It is a most valuable review, and quite up to date, even though the Russian edition was first published in 1956. This English edition incorporates material up to and including the radio astronomy symposium held in Paris in 1958; in addition, it has an appendix which records progress in the field up to April 1960.

Much of the discussion refers to subjects to which the author himself has contributed very greatly, notably synchrotron emission from electrons moving with relativistic energies in magnetic fields and the problem of the generation of cosmic rays in supernovae. The mechanism for emitting

radio waves nonthermally is discussed in great detail, as well as the mechanism for emitting the 21-cm hydrogen line. This sets the stage for a discussion of the structure of our galaxy and its radio spectrum. Next, the special problems associated with supernovae are described, in particular the radio emission of the Crab Nebula and the relation to the origin of cosmic rays. In the final chapter extragalactic radio sources are described.

The translation is smooth and reads easily. A detailed index has been added. The book could have been improved by better reproduction of the photographs.

The emphasis of this volume is definitely on galactic and extra-galactic radio emission, and as a result the radio astronomical work relating to the sun and the planets is not covered. However, in all other respects the present volume sets a landmark in the discussion of galactic radio astronomy. It should be useful, as a textbook for courses in astrophysics, specifically radio astronomy, but it is also written so that the reader with a fairly general scientific background can acquire, quite easily, an intimate knowledge of the present state of research in radio astronomy.

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International Education in Physics. Proceedings of the International Conference on Physics Education. UNESCO House, Paris, 18 July to 4 August 1960. Sanborn C. Brown and Norman Clarke, Eds. Technology Press and Wiley, New York, 1961. 191 pp. $4.50.

In the summer of 1960, 86 physicists met in Paris to confer on physics education. The conference was sponsored by the International Union of Pure and Applied Physics. Immediately after the conference, its chairman, Sanborn C. Brown (Massachusetts Institute of Technology), and its secretary, Norman Clarke (the Institute of Physics and the Physical Society of the United Kingdom), retired to a small English inn to compile the proceedings.

Perhaps the major value of any such conference is to its participants. However, this speedy publication of the proceedings guarantees that the Paris conference will have a significant impact on international physics education.

The book begins with background information on the conference and a list of the 86 delegates representing 32 countries. It then lists the resolutions adopted unanimously by the conference. The conference affirmed the importance of physics "as an essential part of the intellectual life of man at the present day," and recommended that all children should be given some formal education in physics. It recommended that support be given to many attempts to improve physics education and that IUPAP accept responsibility for organizing such support. It recognized that physics education is dependent upon good physics teachers and recommended some measures to increase the supply of teachers. The final resolution raises the hopes of those who were not present by proposing that more such conferences be held from time to time, each to discuss certain aspects of the problems of physics education.

The major part of the proceedings consists of a report on the papers presented at the conference and the ensuing discussion. There are chapters on physics as a part of general education, the roles of examinations and of laboratory work in physics education, selection of students, experimental programs in physics, curriculum content and presentation, training of teachers, problems in teaching physics to nonphysics students, the postgraduate and mathematical education of physicists, and the impact of professional physics organizations.

An American physicist will be surprised to discover how widespread are some of the problems that he considers peculiar to the United States. Even in Switzerland (where most high school physics teachers have the Ph.D. and receive high salaries), teachers are being wooed away from high school teaching into industrial or university careers. And in France, the physicists try to maintain the integrity of their service courses for nonphysicists, putting major emphasis on fundamental principles rather than on practical applications, but they worry about whether mathematicians teach enough mathematics appropriate for physics students. With all the similarities, however, there were many differences in perspective among the various delegates. Physicists concerned about educational problems are well-advised to study carefully the report of the Paris conference.

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Management Sciences, Models and Techniques. vol. 1 and vol. 2. C. West Churchman and Michel Verhulst, Eds. Pergamon, New York, 1960. xxix + 602 pp.; xiv + 511 pp. $30.

Although mathematical analysis has long been applied to certain management problems, it is only since World War II that the systematic integration of scientific techniques into management problems has been a central theme in advanced business thinking. It is interesting just to browse through these volumes, which contain the proceedings of the sixth international meeting of the Institute of Management Sciences, and to note that scarcely any form of business activity has not been scrutinized, subject to optimization, suboptimization, analysis, synthesis, and otherwise put through the operations research wringer. Of course, this development is natural; in a world grown so complex, questions which require quantitative answers require quantitative analysis. But those of us with a touch of the romantic will suffer momentary regret that the daring entrepeneur is to be replaced by the unfailingly conservative computer.

It is not possible to comment on any sizable number of the papers presented, but I will try to mention some of the major trends in current management research, as evidenced by these reports. The greatest interest is presently being accorded to management gaming and business simulation. These are old and honorable aids of the military but new to management, and consequently they are under extensive and enthusiastic development.

Perhaps the most novel of the papers presented is one on psychoanalytic processes in management, relating individual management thought and behavior patterns to the business policies of several firms. This approach may well prove to be a major tool in the investigation of outstanding problems in decision theory. The problems themselves are illustrated by the perversely nonoptimal behavior of the subjects in an experimental investigation of nonzero-sum two-person games. Other studies include more conventional applications of operations research techniques, but the topics themselves suggest some of the fascination of management problems. These range from a study of the problems of a small farmer to the role of scientific management in promoting the growth of underdeveloped countries. Beside these, there

are others on applications of control theory, queuing theory, linear and nonlinear programming, and inventory theory.

In summary, management science is still in a transient phase in which the profitable lines of investigation are not entirely distinguished from the unprofitable ones. This initial exuberance will pass in time, but one hopes that not too much time will be taken up with such meaningless questions (this one raised by a past president of TIMS) as whether management sciences are part of the natural or the social sciences.

GEORGE WEISS
University of Maryland

The Orion Book of the Sun. Étienne Lalou (translated from *Le Soleil*). 110 pp. **The Orion Book of Time**. François le Lionnais (translated from *Le Temps*). 110 pp. **The Orion Book of the Sky**. Jean-Claude Pecker (translated from *Le Ciel*). 113 pp. Orion Press, New York, 1960 (all translations by William D. O'Gorman, Jr.). Illus. $6.95 each.

The magic borderland between the sciences and the visual arts has remained curiously unexploited. One would have thought that the growing tips of man's intellectual and artistic creativities would have fused more frequently. In spite of the publication in portfolios of several collections illustrating "science in art" and "art in science" as well as an embarrassment caused by the exploitation of Madison Avenue art in scientific advertising, all efforts seem to pale into rather unbeautiful science, or into rather unscientific art, when compared with this new series of books, published originally in French but now translated into English.

Each book takes its stated topic and discourses upon it with an unusually lively range of perspectives. The sun is seen in a child's painting, in pagan rites, classical learning, medieval miniatures, and glowing brass orreries, and in an architectonic solar furnace. Time takes its shape from fossils and allegory, sundials, delicately jeweled clockworks, and the rhythmic flapping of a bird's wings. We look at the sky through the eyes of aborigines and Egyptians, Christians and Moslems, contrasting the inner sight of Hieronymus Bosch and Joan Miro with the gulping objectivity of Galileo and Mount Palomar.

For the scintillating beauty and brilliant quality of their color prints and line illustrations, these books raise the standards of the field by a whole order of magnitude. Nothing like them has been seen before, and the effect of the books, at least on this visual-thinking reviewer, has been so intense and traumatic that I must strongly recommend that all other scientists who are in the category that James Clerk Maxwell once described as "visible-image worshippers" obtain these books and repeat the experience for themselves.

The pictures are, after all, the main part of these books, and all else around them must seem relatively insignificant. It would be fine glory if the text of the book and its production, apart from the art work, were similarly praiseworthy. Alas, such is not the case. The original texts may have been good, in that poetic, philosophic, grand style of writing that is so peculiarly French. The English version, however, appears stilted and somewhat amateurish, replete with Gallicisms and unworthy to its purpose. A few direct errors of historic or scientific expertise also exist, and some of the illustrations seem to be wrongly described. The latter point is made much more difficult by the unscholarly method of citing only photographic credits and descriptive captions for these magnificent pictures; in many cases this makes it quite impossible to learn *exactly* where an illustration came from and what it represents.

Although in most features of design and production the book is impeccable, its format is slightly inconvenient; but this seems to be imposed by the size of the pictures and may be forgiven the editors. In all, the art work, especially that of the color photographs, is so magnificently reproduced that the faults of the texts, the shapes of the books, and the relatively high price can hardly matter. I hope that perhaps sometime in the future Orion Press can be prevailed upon to collect the most striking pictures and reproduce a big book full of them, perhaps with short captions instead of a text, and with a list of sources for those whose curiosity might tempt them to find out how so much beautiful art came out of and into science.

DEREK J. DE SOLLA PRICE
Department of the History of Science and Medicine, Yale University

Motion and Relativity (Polish Academy of Sciences, Physical Monographs). Leopold Infeld and Jerzy Pebański. Pergamon, New York; Państwowe Wydawnictwo Naukowe, Warsaw, Poland, 1961. 229 pp. Illus. $7.50.

The senior author of this monograph is Leopold Infeld, coauthor of the famous paper by A. Einstein, L. Infeld, and B. Hoffmann [*Annals of Mathematics* **39**, 65 (1938)], which heralded the modern theory of the ponderomotive laws in the general theory of relativity. For the nonspecialist it might be remarked that modern field theories, such as Maxwell's theory of the electromagnetic field, generally contain two entirely different kinds of dynamical laws, the field equations proper and the ponderomotive laws. The field equations proper describe the development of the field, both in terms of the distribution of its sources (such as the electric charges and currents) and in terms of its internal dynamics. The ponderomotive laws, or the equations of motion, delineate the forces that a particle experiences by being immersed in the field. Of all known field theories, the general theory of relativity alone establishes a logical interdependence of these two kinds of law, in that the field equations cannot be solved unless the sources of the field, constituting the ponderable matter, satisfy the equations of motion.

Because of the considerable complexity of the mathematics of general relativity, the pioneer paper by Einstein, Infeld, and Hoffmann involved such a body of arithmetic that the published version presented the formal work only in outline and referred the reader to a more extensive documentation deposited in the library of the Institute for Advanced Study (Princeton, New Jersey). Subsequent analysis of the underlying mathematical and physical concepts led to a gradual reduction in the amount of labor required and, with this simplification, to the possibility of penetrating more deeply. The present book gives a lucid review of the present stage of the theory of motion.

Some of the treatments given appear controversial, in particular the discussion of radiation in chapter 4. Nevertheless, this excellent presentation by acknowledged authorities will be of great value to all who work in the field.

PETER G. BERGMANN
Department of Physics, Syracuse University

Industrial Research Laboratories of the United States. Publication No. 844. Compiled by John H. Gribbin and Sue Singer Krogfus. National Academy of Sciences–National Research Council, Washington, D.C., ed. 11, 1961. x + 698 pp. $12.

A compilation listing 5420 nongovernmental laboratories devoted to fundamental and applied research. The information, gathered in late 1959 and early 1960, was verified between May and July 1960. Emphasis was placed on listing subsidiaries under parent firms. This edition includes a useful 20,000-entry name index of personnel. The entries, arranged alphabetically, supply the following information: mailing address; name of the president; research staff—name of the director (usually) and the number and type of staff employed; and research areas.

Viscoelastic Properties of Polymers. John D. Ferry. Wiley, New York, 1961. xx + 482 pp. Illus. $15.

This volume is the latest of three or four important works, devoted in whole or in large part to the viscoelastic properties of polymers, to be published in the United States during the past 12 months. In writing this book, Ferry set for himself four goals: assembling in one place of the informational tools needed by workers in the field to make measurements and interpret data; the straightforward exposition of the concepts of viscoelasticity from the ground up, for the benefit of beginners in the field; the delineation of areas requiring further theoretical and experimental work; and finally, the illustration of applications of viscoelastic functions and the reduced-variable technique to technological situations. This program is not necessarily as diversified and, perhaps, overambitious as it might seem, for these goals are not unrelated. For example, one could not get very far toward the second objective without accomplishing the first.

It will not surprise his colleagues to find that John Ferry develops his subject with a logical rigor that approaches the proverbial Lagrangian elegance about as closely as could any treatment of viscoelasticity, for the subject is still in its adolescence. Although it is logically coherent, this work is not a compilation of all work that has been done on the viscoelastic properties of polymers, nor is it a stringing together of chapters on nonsequential, specialized subjects. Perhaps because he wished to adhere closely to this tone of coherence, the author has been selective. This has resulted in what some might consider important omissions. For example, there is only an allusion to the Eyring-Tobolsky reaction-rate theory of viscoelastic deformation, and there is no reference whatever to the James-Guth theory of rubber-like elasticity.

W. JAMES LYONS
Textile Research Institute, Princeton, N.J.

Digital Computers and Nuclear Reactor Calculations. Ward C. Sangren. Wiley, New York, 1960. xi + 208 pp. $8.50.

The past two decades have seen the birth and rapid parallel development of both high-speed digital computers and nuclear reactors. During the past decade it has become standard procedure to use digital computers for the solution of problems arising in nearly every phase of nuclear reactor design and application. A significant proportion of the available time on our largest and fastest digital computers is devoted specifically to calculations in these areas. Ward Sangren has long been associated with the application of digital computers to nuclear reactor calculations and thus is eminently qualified to write in this field. He states that a primary objective of his book is "to present the nuclear engineer or scientist with an introduction to high speed nuclear reactor calculations." The topics covered range over a broad field. Chapter headings include "Digital computers," "Programming," "Numerical analysis," "A code for fission product poisoning," "Diffusion and age diffusion calculations," "Transport equation—Monte Carlo," and "Additional reactor calculations." Each chapter is self-contained and can be read independently of the others.

The introduction reviews briefly the history of nuclear reactor and digital computer developments. Then surveys are given of the problems involved in building nuclear reactors and in solving problems on digital computers. Two chapters are devoted to a general description of digital computers and the preparation of computer programs. There is a chapter on the language and methods of numerical analysis. As a transition to the last chapters, concerned with nuclear reactor calculations, there is a chapter which describes the various steps involved in setting up and solving a particular problem on a digital computer.

Because the range of subjects covered is so broad and the fields involved are developing so rapidly, Sangren has been able to give only a brief introduction to any specific topic.

ELIZABETH CUTHILL
Applied Mathematics Laboratory, David Taylor Model Basin

New Books

Biological and Medical Sciences

Advances in Food Research. vol. 10. C. O. Chinchester, E. M. Mrak, and G. F. Stewart, Eds. Academic Press, New York, 1960. 501 pp. $13.

Advances in Veterinary Science. vol. 6. C. A. Brandley and E. L. Jungherr, Eds. Academic Press, New York, 1960. 393 pp. Illus. $12.

Advances in Virus Research. vol. 7. Kenneth M. Smith and Max A. Lauffer, Eds. Academic Press, New York, 1960. 406 pp. $10.

The Amazing World of Medicine. Helen Wright and Samuel Rapport. Harper, New York, 1961. 311 pp. $3.50.

Animal Husbandry Heresies. Allan Fraser. Crosby Lockwood, London, 1960. 200 pp.

Animal Parasites in Man. N. H. Swellengrebel and M. M. Sterman. Van Nostrand, Princeton, N.J., 1961. 662 pp. Illus. $12.

Annual Review of Entomology. vol. 6. Edward A. Steinhaus and Ray F. Smith, Eds. Annual Reviews, Palo Alto, Calif., 1961. 478 pp. $7.

Bacterial Plant Pathogens. C. Stapp. Translated by A. Schoenfeld. Oxford Univ. Press, New York. 1961. (Translated from *Pflanzenpathogen Bakterian*, Parey, Berlin, 1958.) 320 pp. Illus. $6.75.

Bailliere's Atlas of Female Anatomy. Revised by Katharine F. Armstrong. Bailliere, Tindall and Cox, London; Williams and Wilkins, Baltimore, Md., ed. 6, 1960. 30 pp. + plates. $3.50.

Biochemistry for Medical Students. William Veale Thorpe. Churchill, London; Little, Brown, Boston, Mass., ed. 7, 1960. 560 pp. Illus. $9.

Biological Degradation of Cellulose. J. A. Gascoigne and Margaret M. Gascoigne. Butterworths, Washington, D.C., 1960. 270 pp. Illus. $8.

Carcinogenesis. I. Hieger. Academic Press, New York, 1961. 149 pp. $5.

Cell and Tissue Culture. John Paul. Williams and Wilkins, Baltimore, Md., 1960. 323 pp. Illus. $7.50.

Clinical Pharmacology. D. R. Laurence and R. Moulton. Little, Brown, Boston, Mass., 1960. 498 pp. $10.

The Choice of a Medical Career. Essays on the fields of medicine. Joseph Garland and Joseph Stoke III, Eds. Lippincott, Philadelphia, Pa., 1961. 241 pp. $5.

Clinical Toxicology. C. J. Polson and R. N. Tattersall. Lippincott, Philadelphia, Pa., 1959. 600 pp. $10. Describes the clinical features of poisoning in the home, covering common poisons and others of practical importance. The bibliography is selective.

The Crisis in American Medicine. Marion K. Sanders, Ed. Harper, New York, 1961. 159 pp. $3.75.

Essentials of Human Anatomy. Russell T. Woodburne. Oxford Univ. Press, New York, ed. 2, 1961. $12.50.

Fossil Collecting. An illustrated guide. Richard Casanova. Vinson Brown, Ed. Faber and Faber, London, 1960. 18s.

Free Radicals in Biological Systems. M. S. Blois *et al.*, Eds. Academic Press, New York, 1961. 405 pp. Illus. $14.50.

The Glycolysis and Respiration of Tumors. Alan C. Aisenberg. Academic Press, New York, 1961. 237 pp. $8.

Handbook of Physiology. R. J. S. McDowall. Lippincott, Philadelphia, Pa., ed. 43, 1960. 769 pp. Illus. $12.50.

The Harvey Lectures. Academic Press, New York, 1961. 273 pp. Illus. $8. Series 55, 1959–60.

Heathland Ecology. C. P. Friedlander. Harvard Univ. Press, Cambridge, Mass., 1961. 104 pp. Illus. $1.75.

How To Know the Butterflies. Paul R. Ehrlich and Anne H. Ehrlich. Brown, Dubuque, Iowa, 1961. 262 pp. Illus. Illustrated keys to species found in North America, north of Mexico.

Human Factors in Jet and Space Travel. A medical-psychological analysis. S. B. Sells and Charles A. Berry. Ronald, New York, 1961. 402 pp. $12.

The Human Organism. Russell Myles De Coursey. McGraw-Hill, New York, ed. 2, 1961. 671 pp. Illus. $7.50.

Economics and the Social Sciences

Advances in Psychosomatic Medicine. Arthur Jores and Hellmuth Freyberger, Eds. Brunner, New York, 1961 (order from Basic Books, New York). 366 pp. Illus. $8.50. Symposium of the Fourth European Conference on Psychosomatic Research, 1959.

Delinquent and Neurotic Children. A comparative study. Ivy Bennett. Basic Books, New York, 1960; Tavistock, London, 1959. 545 pp. $10.

The Encyclopedia of Sexual Behavior. vols. 1 and 2. Albert Ellis and Albert Abarbanel, Eds. Hawthorn Books, New York, 1961. 1059 pp. $30.

Geographisches Taschenbuch. Und Jahrweiser zur Landeskunde 1960/61. E. Meynen, Ed. Franz Steiner, Wiesbaden, Germany, 1961. 589 pp. Illus. + plates. DM. 14.80.

A Geography of Norden. Denmark, Finland, Iceland, Norway, and Sweden. Alex Sømme, Ed. Cappelens, Oslo, Norway; Heinemann, London, 1960. 360 pp. Plates.

Growth and Stability of the Postwar Economy. Bert G. Hickman. Brookings Institution, Washington, D.C., 1961. 444 pp. Illus. $6.

An Introduction to Psychoanalytic Theory of Motivation. Walter Toman. Pergamon, New York, 1960. 363 pp. $9.

Northwest Ethiopia. Peoples and economy. Frederick J. Simoons. Univ. of Wisconsin Press, Madison, 1960. 267 pp. Illus. + plates. $5.

Psychology. An introduction to the study of human behavior. Henry Clay Lindgren and Donn Byrne. Wiley, New York, 1961. 440 pp. Illus. $6.50

Psychology: A Problem-Solving Approach. Donald M. Johnson, Harper, New York, 1961. 596 pp. Illus. $7.

Psychophysiological Aspects of Space Flight. Bernard E. Flaherty, Ed. Columbia Univ. Press, New York, 1961. 403 pp. Illus. $10. Papers presented at a symposium sponsored by the School of Aviation Medicine, U.S. Air Force Aerospace Medical Center, held in May 1960.

The Psychotherapy Relationship. William U. Snyder and B. June Snyder. Macmillan, New York, 1961. 430 pp. $7.50.

Psychotherapy with Schizophrenics. A reappraisal. Joseph G. Dawson, Herbert K. Stone, and Nicholas P. Dellis, Eds. Louisiana State Univ. Press, Baton Rouge, 1961. 168 pp. $5. The views of a group of practicing psychotherapists regarding treatment of schizophrenics.

River Basin Surveys Papers. Inter-Agency Archeological Salvage Program, Nos. 15-20. Frank H. H. Roberts, Jr, Ed. Bureau of American Ethnology, Smithsonian Institution, Washington, D.C., 1960. 347 pp. Illus. + plates.

Some Reflections on Genius. And other essays. Russell Brain. Lippincott, Philadelphia, Pa., 1960. 192 pp. $6.

The Soviet Union. The land and its people. Georges Jorré. Translated and revised by E. D. Laborde. Longmans, Green, New York, ed. 2, 1961. 392 pp. Illus. $7.50.

Trade with Communist Countries. Alec Nove and Desmond Donnelly. Published for the Institute of Economic Affairs by Macmillan, New York, 1961. 183 pp. Illus. $6.

The Training of Psychotherapists. A multidisciplinary approach. Nicholas P. Dellis and Herbert K. Stone, Eds. Louisiana State Univ. Press, Baton Rouge, 1961. 207 pp. $5. Papers resulting from a symposium held 11–13 November 1959.

Mathematics, Physical Sciences, and Engineering

The Algebra of Probable Inference. Richard T. Cox. Johns Hopkins Press, Baltimore, Md., 1961. 124 pp. $5.

The Application of Group Theory in Physics. G. Ya. Lyubarskii. Translated from the Russian by Stevan Dedijer. Pergamon, New York, 1960. 390 pp. $10.

The Atom and Its Nucleus. George Gamow. Prentice-Hall, Englewood Cliffs, N.J., 1961. 159 pp. Illus. Paper, $1.95; cloth, $3.75.

Basic Principles of Fission Reactors. W. R. Harper. Interscience, New York, 1961. 322 pp. Illus. $7.50

Cahiers de Synthèse Organique. Méthods et tableaux d'application. Léon Velluz, Ed. Jean Mathieu, André Allais, and Jacques Valls. vol. 6, 418 pp., paper, NF. 110; cloth, NF. 120. vol. 7, 309 pp., paper, NF. 100; cloth, NF. 110. vol. 8, 233 pp., paper, NF. 75; cloth, NF. 85. Masson, Paris, 1961.

Chemical Engineering Practice. vol. 10, *Ancillary Services.* Herbert W. Cremer and Sidney B. Watkins, Eds. Academic Press, New York; Butterworths, London, 1960. 632 pp. $19.50.

The Chemical Warfare Service: From Laboratory to Field. Leo P. Brophy, Wyndham D. Miles, and Rexmond C. Cochrane. Office of the Chief of Military History, Department of the Army, Washington, D.C., 1959 (order from Supt. of Documents, GPO, Washington 25). 516 pp. Illus. $3.50.

Chemistry, Man's Servant. Leonard J. Fliedner and Louis Teichman. Allyn and Bacon, Boston, Mass., 1961. 635 pp. Illus.

The Creation of the Universe. George Gamow. Viking, New York, ed. 2, 1961. 159 pp. Illus. $4.50.

Digital Computer and Control Engineering. Robert Steven Ledley. McGraw-Hill, New York, 1960. 859 pp. Illus. $14.50.

Electromagnetic Structure of Nucleons. S. D. Drell and F. Zachariasen. Oxford Univ. Press, New York, 1961. 117 pp. Illus. $2.

Electronics. Paul M. Chirlian and Armen H. Zemanian. McGraw-Hill, New York, 1961. 347 pp. Illus. $8.75

Elements of Modern Physics. A course in those aspects of modern physics that underlie important engineering developments. Paul L. Copeland and William E. Bennett. Oxford Univ. Press, New York, 1961. 517 pp. Illus. $8.50.

The Fundamental Atomic Constants. J. H. Sanders. Oxford Univ. Press, New York, 1961. 96 pp. Paper, $1.60.

Gas Chromatography. Henry J. Noebels, F. R. Wall, and Nathaniel Brenner, Eds. Academic Press, New York, 1961. 479 pp. Illus. $16. Proceedings of the second international symposium, held at Michigan State University, in June 1959.

Gmelins Handbuch der Anorganischen Chemie. pt. B, section 2, System No. 9, *Sulphur*, 758 pp., illus., paper, $111; cloth, $112. System No. 20, *Lithium* (supplement volume), 525 pp., illus., paper, $77.50; cloth, $78.50. Section 1, System No. 34, *Mercury*, 466 pp., paper, $67.50. Verlag Chemie, Weinheim, Germany, 1960.

Heterocyclic Systems with Bridgehead Nitrogen Atoms. pt. 1. William L. Mosby. Interscience, New York, 1961. 757 pp. Illus. $48 (by subscription, $43). vol. 15, *Chemistry of Heterocyclic Compounds.*

High Temperature Metallography. M. G. Lozinskii. Translated by L. Herdan. Pergamon, New York, 1961. 504 pp. Illus. $17.50.

Hydrometry. Theory and practice of hydraulic measurements. Adam T. Troskolański. Pergamon, New York; Panstwowe Wydawnictwa Techniczne, Warsaw, Poland, 1960. 704 pp. Illus. $18. Revised and enlarged from the first Polish edition.

International Summer Course in Plasma Physics, 1960. Material presented at the course. Risö Rept. No. 18. Research Establishment Risö. Danish Atomic Energy Commission, Roskilde, Denmark, 1960. 649 pp. Illus. Kr. 60.

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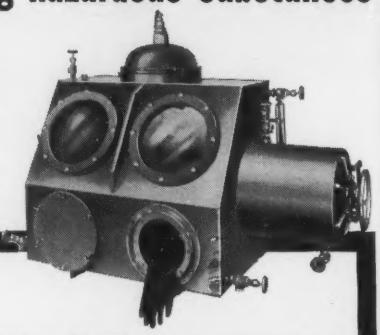

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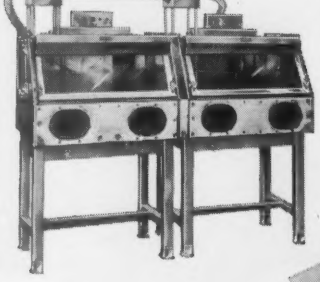

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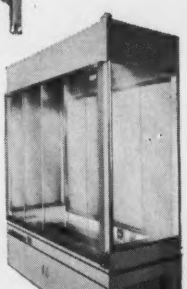

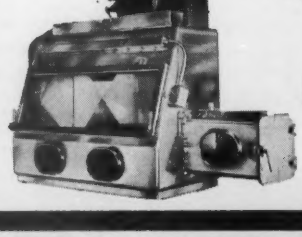

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Anthropology and Archeology

Africa Today and Tomorrow, J. Hatch (Praeger), 16 Sept. 1960, 731

The Antecedents of Man, W. E. Le Gros Clark (Quadrangle Books), 22 Apr. 1960, 1208

Anthropological Papers, Bureau of American Ethnology (Smithsonian Institution), 30 Sept. 1960, 888

Approach to Archaeology, S. Piggott (Harvard Univ. Press), 27 May 1960, 1605

Beyond the Mountains of the Moon, E. H. Winter (Univ. of Illinois Press), 22 Apr. 1960, 1207

China, Chang-tu Hu *et al.* (Hraf Press, New Haven), 6 May 1960, 1366

Cochiti, C. H. Lange (Univ. of Texas Press), 5 Aug. 1960, 344

In the Company of Man, J. B. Casagrande, Ed. (Harper), 30 Sept. 1960, 889

Contributions to the Physical Anthropology of the Soviet Union, V. V. Bunak *et al.* (Peabody Museum), 22 Apr. 1960, 1210

Cultures and Societies of Africa, S. Ottenberg and P. Ottenberg (Random House), 27 Jan. 1961, 272

Dictionary of the American Indian, J. L. Stoutenburgh, Jr. (Philosophical Library), 20 May 1960, 1521

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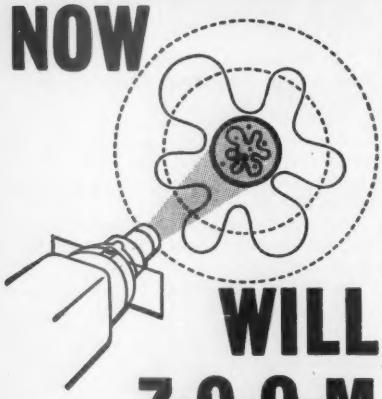

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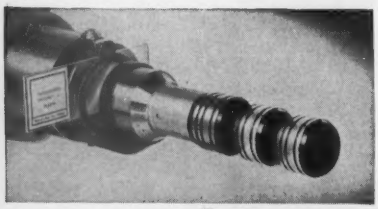

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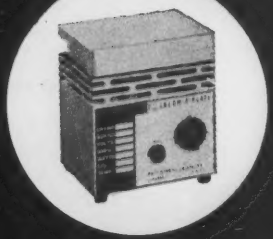

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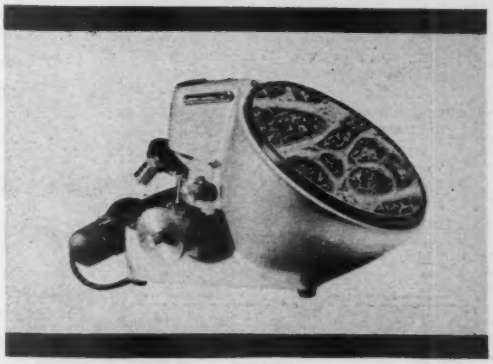

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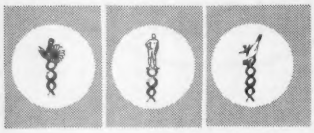

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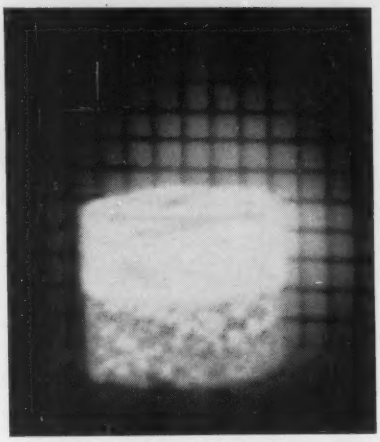

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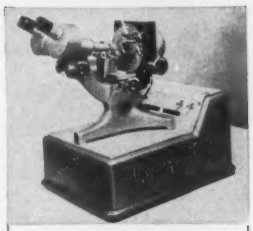

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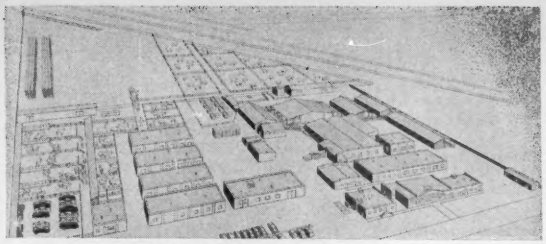

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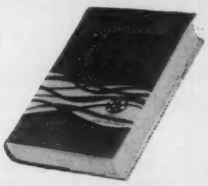

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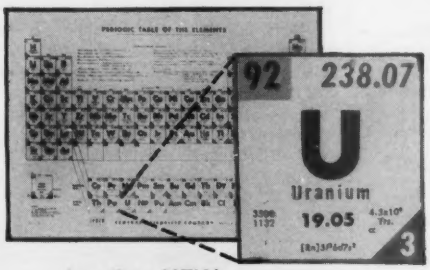

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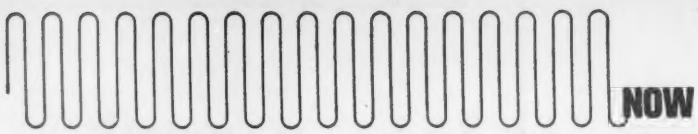

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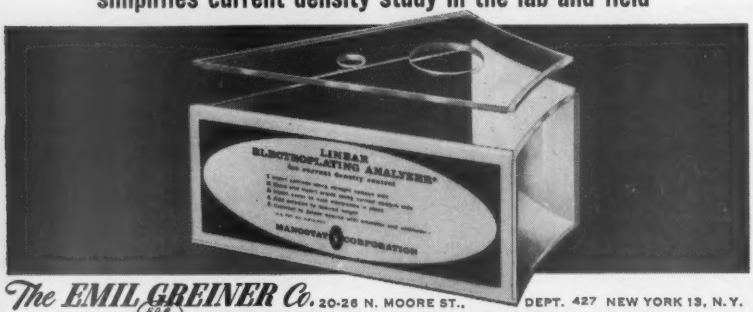

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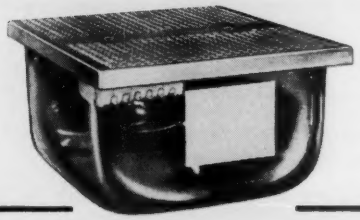

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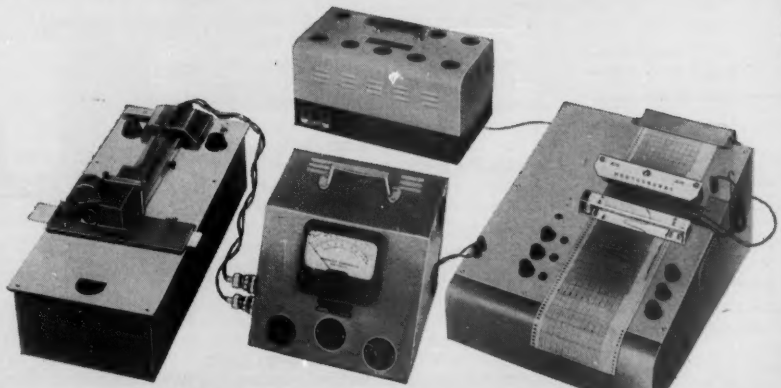

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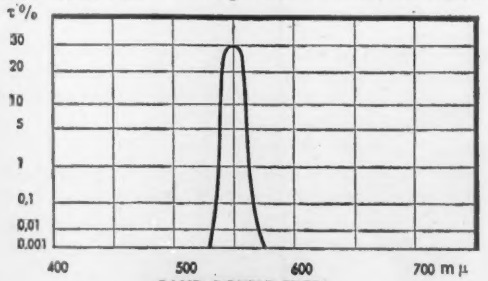

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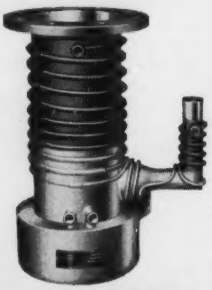

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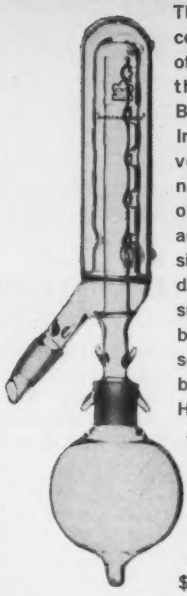

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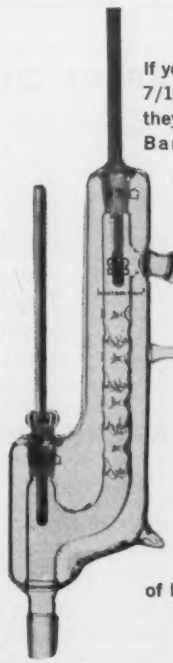

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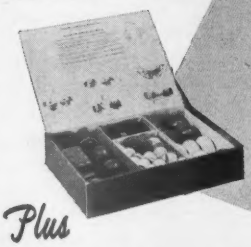

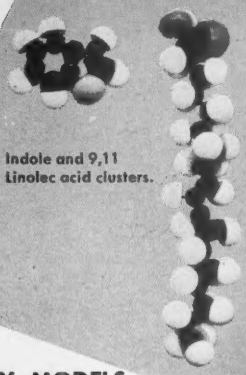

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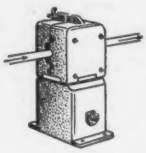

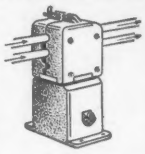

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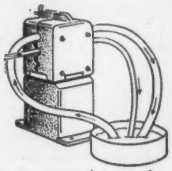

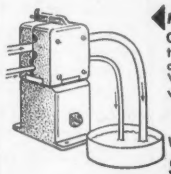

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Letters

"Backlash"

Something more than a year ago one of your editorialists offered wry commentary on the film *On the Beach* [*Science* **130**, 1679 (1959)]. At that time it struck me as most unfortunate that the editorialist devoted all his attention to the dramatic and emotional content of this movie without the slightest suggestion that the film was based upon a scientifically absurd plot. In the year that has elapsed, *On the Beach* has had whatever impact it was capable of having. I believe that, on balance, the film has had a subtly unfortunate impact, and that at least brief discussion of its absurdity from the standpoint of radiology is now very much in order.

As mere symbolism about man's desperate plight in an age of ever more awesome military technology, the film's story of slow but sure extinction in Australia resulting from a nuclear war in the Northern Hemisphere may be acceptable, but *only* in a symbolic sense. To most laymen walking out of the movie houses, however, the story surely came through as a dramatic interpretation of what *could* happen. My own inquiries among residents of Tucson who saw this film lead me to conclude that many in the audiences have since gone on to an exceedingly dangerous inference: If nuclear war would be so totally lethal, no country could be so irrational as to start a nuclear war; hence, nuclear war has its own built-in deterrent. That is, if Australians in the Southern Hemisphere could die like flies when the fallout wafted across the Equator a few months after attack, then clearly all the Northern Hemisphere aggressor's population would have been lethally irradiated weeks earlier. And this being the case, who would ever elect to start such a suicidal action?

The melancholy fact, which all of us should clearly realize, is that the alleged "backlash effect" on a would-be nuclear aggressor would be by no means large enough to constitute a powerful deterrent: *On the Beach* was in error by many orders of magnitude.

To see this, consider a 20,000-megaton nuclear war in the Northern Hemisphere (five times greater than the hypothetical nuclear war considered in the latest Office of Civil and Defense Mobilization paper exercises). And, to give death the benefit of all possible doubt, assume the weapons to be 100-percent fission weapons, rather than the less deadly 50-50 weapons assumed in recent analysis of hypothetical attacks. Then, scaling up the calculations summarized in 1959 in expert testimony before the Joint Congressional Committee on Atomic Energy [*Biological and Environmental Effects of Nuclear War* (Government Printing Office, Washington, D.C., 1959), especially pages 464–469], we can readily obtain a quantitative estimate of the backlash-cost of nuclear aggression.

If 80 percent of the fission yield descends as local fallout in the *target* areas (we need not specify for the moment whether the 20,000 megatons all fall on the victim country or are split in some way between aggressor and victim, for our concern is with prolonged world-wide fallout), and if we assume, like Machta, Dunning, and other contributors to the cited Congressional testimony, that 75 percent of the residual world-wide fallout is deposited in the latitude belt from 30° to 60°N, then we find that the total external and internal irradiation in the next *35 years* after the attack would induce about 250,000 leukemias per 200 million of survivors in the Northern Hemisphere and 50,000 bone tumors per 200 million survivors, when, in order to maximize the effects, no thresholds are assumed. (As for the dosage magnitudes underlying the above estimates, it may be noted that almost exactly half the leukemias are due to strontium-90, which, according to the model used here, is deposited over the middle-latitude zone, including the attacker's homeland, with a density of 8 curies per square mile, yielding about 800 strontium units in man at the end of the food chain.) Just over 1 million tangible genetic defects would be mutationally induced during the roughly 35 years required for 200 million survivors to yield 200 million live births (the corresponding natural-incidence figures for 200 millions over 35 years are about 400,000 leukemias, 70,000 bone tumors, and 4 million tangible mutations).

Now, if all the 20,000 megatons went off in the victim country and the aggressor's untouched population were just 200 million, the *upper* limit to the backlash-cost of aggression, which is the chief concern of this letter, is provided by the figures given above. Horrible as is the human meaning of those numbers, we must recognize that the price is such as to be considered negligible by any and all *military* standards. The price Germany and Japan paid for electing aggression in World War II—a total of 4 million German and Japanese military deaths, paid out all in one comparatively short period—was so very much greater than the backlash price of nuclear aggression, amortized over a 35-year period, as to make it starkly obvious that no built-in deterrent of the kind suggested by *On the Beach* can be relied upon to protect the world from nuclear aggression.

Instead, the actual situation appears to be one in which weapon technology is rendering aggression more and more likely on the terribly simple ground that, in a period of great international tension, a worried nuclear power may feel it cannot afford *not* to strike first. Thus, *On the Beach* produced a comforting but extremely dangerous misinference. A massive nuclear attack would not produce a backlash of fallout of deterrent magnitude on an aggressor country that lay thousands of miles downwind from the target country. Neville Shute and Hollywood widely missed the mark, and I fear that more than a little mischief has been done thereby.

JAMES E. MCDONALD
University of Arizona, Tucson

The Scientist and the Dominant Danger

I would like to comment on the address by Sir Charles Snow [*Science* **133**, 256 (27 Jan. 1961)]. It seems to me that the meat of his remarks may be summarized in two statements. First: Scientists have direct technical knowledge in areas of political relevance, and since they are required by training to be moral individuals, they must accept a special responsibility—a greater one than that of mere citizens. Second: Without test cessation we face a certainty of disaster, while with it, we have a chance. (The implicit conclusion is that Western scientists must with great verve lead their countries to permanent and, if necessary, unilateral cessation of nuclear weapons testing.)

I have not observed that scientists are any more or less moral outside of their metier than are professional people generally. May I remind Sir Charles that Benjamin Thompson (Count Rumford) was both a notable

scientist and a thoroughgoing scoundrel. Closer to our times, where was scientific honesty in F. Joliot's promotion of the germ-warfare fabrication in 1952? Didn't scientists participate in Nazi atrocity experiments or in Soviet brainwashing? On a more mundane level, I find scientists holding all manner of religious and antireligious beliefs, having all manner of political affiliations, just as do lawyers, doctors, and the rest. The ethical standards in scientific work are real (are they not also in other professions?), but that does not seem to keep us from having our full share of false prophets.

The supposition that particular technical knowledge possessed by scientists gives us a special claim to the role of leader (as opposed to technical adviser) I regard as silly, just as I regard parallel claims that generals and admirals should determine over-all war policy, that doctors and psychiatrists should decide on candidacy for sterlization or euthanasia, or that lawyers should rule on capital punishment.

I do believe that scientists should take active roles in community and national affairs, perhaps especially in areas where their technical knowledge and habit of open discussion is of help. Let this be done, however, with innate humility, with complete respect for other educated and intelligent people, and with no prior assumption that the measured opinions of the nonscientists are less worthy than those of the scientist.

I would not be so concerned with this first aspect of Sir Charles's address were it not for the second, and its implicit conclusion. In it I detect the not uncommon condition of 20/20 vision in viewing Nazi horrors and decided myopia with respect to Soviet ones. Should we not shudder as much at the slaughter of peasants as at the cremation of Jews? as much at the Hungarian massacre as at that of Lidici? Do we not deserve, even from an ex-scientist, as realistic and frank an appraisal of current evils as of past ones?

It is only realistic to see that, in test cessation negotiations, the horrible risk is that we will make nontrivial concessions and divorce the subject from general disarmament, giving an unretrievable military advantage to an undeniably hostile, amoral, and imperialistic regime. The consequences are ones the Hungarians, at least, well understand.

On the other hand, I find the so-called *N*-nation problem largely irrelevant. The current test moratorium did not prevent France and will not prevent Mainland China and other nations from scratching together some nuclear devices. Really, this is a minor hazard as compared to that of the large stockpile, the effective delivery system, and the freedom from moral restraint of the Soviets. In the meantime, the overblown concern with the *N*-nation, as well as with the fallout, problem is steadily deflecting us from a clear and resolute facing of what is actually the dominant danger to the world today: Soviet power and aggressive intent.

ARTHUR W. ADAMSON
Department of Chemistry, University of Southern California, Los Angeles

I do not propose to reply piecemeal to comments on my article, certainly not to comments of this kind. When I am in the United States in April I shall probably take the occasion, in a lecture, to have a look at the problem in the light of what has been said since.

During that period there is going to appear an authoritative critical analysis of the kind of military review which I suspect Adamson supports. I would like this military analysis (which comes from a much more impressive source than mine) to be in the common pool before I have another go at the argument.

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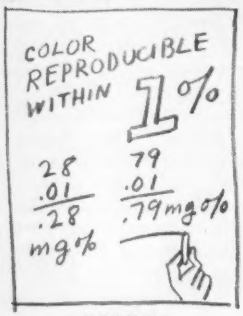

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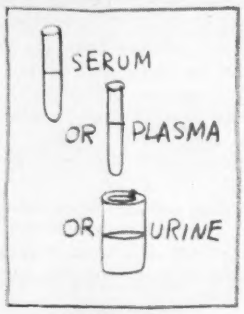

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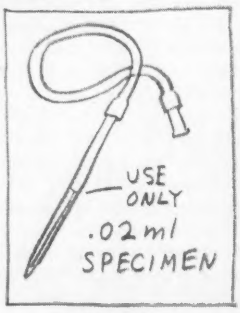

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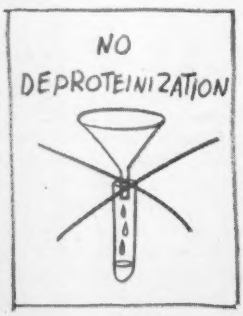

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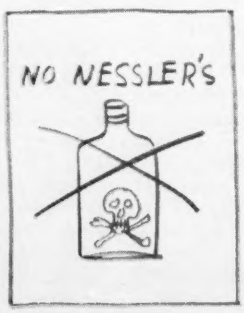

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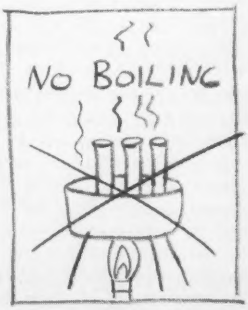

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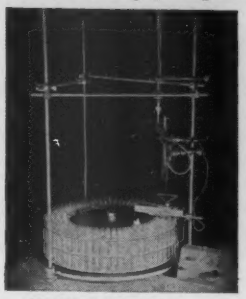

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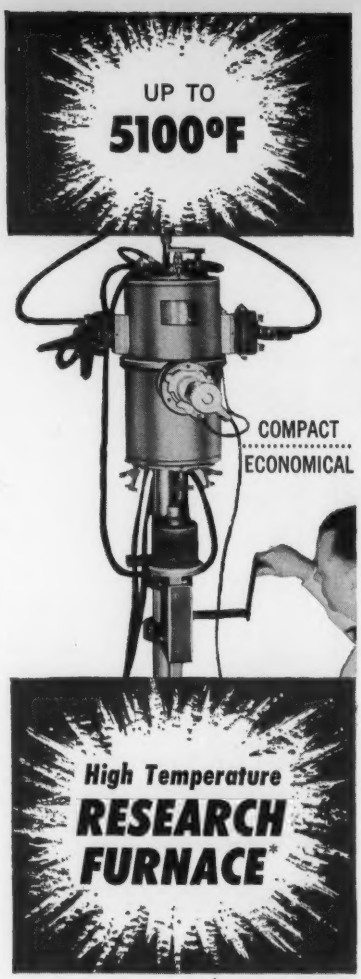
Effective Use of Information

In the course of a study of the U.S. system for handling science and engineering information, the tremendous influence of the educational techniques employed in the training of U.S. scientists and engineers has become very apparent. Radical improvements in the information handling system are hamstrung by the educational system, which turns out a professional scientist or engineer accustomed to acquiring information by the complex, diffuse, and grossly inefficient techniques now in use. Habit patterns thus established in the schools are difficult to change.

The fruits of scientific and engineering discoveries (indeed, their very justification) lie in their application to mankind's daily problems, and the major stumbling blocks in the way of progress are human beings habituated to inefficient methods of acquiring, using, and disseminating information, floundering in an ever-growing morass of technical information. There seems little ground for hope that the flood of technical information will return quietly to its former, confined channels; the volume of published literature is doubling every decade or less. Piecemeal attempts to shore up the present information system will be helpful, but only for a short time. What is needed is a revolution in the training of scientists and engineers, so that their abilities to acquire information of a directly pertinent nature will be multiplied several-fold. I believe the average technically trained man could triple his productivity if he had a more efficient information system at his disposal, and if he were motivated and trained to use it.

It is true that some highly creative research workers on the frontiers of science are productive, even with the present inefficient information system. The numbers of people in these activities are, however, relatively small, and it is conceivable that even their creative productivity could be enhanced by better methods of acquiring and using available information. It is also true that groups of research scientists working with very large, expensive, experimental equipment such as linear accelerators probably know many workers engaged in similar research and exchange information with them informally. While this is true at one period in the history of any development, these frontier areas of science soon become like other well-established areas.

The incentive for a radical improvement in U.S. methods of handling technical information lies, of course, in the need for maximum utilization of the relatively few scientists and engi-



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neers, in whose hands in large measure lies the national security. The extraordinary emphasis in the U.S.S.R. on training technicians to apply available information is well known. The emphasis which is given by the U.S.S.R. to technical information at the State Council of Ministers' level is also well known (even if it is not so well known that the centralized information structure is performing erratically). The United States was founded to give the individual room for maximum development, and its educational system is based on giving each person an opportunity to develop his or her talents to the maximum extent possible. The United States might well make a large national effort to discover and apply new and better techniques for training individuals in the most effective use of information, and then to devise an information system which can serve these better-developed citizens.

W. T. KNOX

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It is true that erroneous and premature publication harms the author and makes his institution ridiculous, but that is a small matter compared with the good effects our present editorial system has on science in general. As a rule, the journals will consider a manuscript regardless of who the author is and will see that it is reviewed by experts. If these men think it is good, it will be published. If they think it is bad, and the author isn't convinced, he can get the manuscript back and try a different journal. It is surprising that editors should undervalue their own role in the scientific process and should seek to legitimize a veto power by presidents, deans, and department heads.

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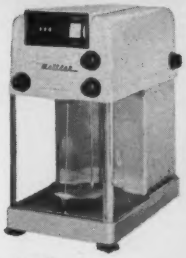

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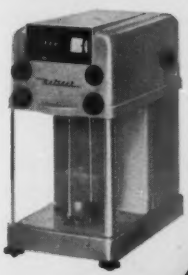

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practiced it usually exists in combination with an authoritarian constitution under which the department chairman is appointed for life and already holds a veto over pay raises and promotions. If he alone also controls the department budget, makes assignments of office and laboratory space, and decides on teaching assignments, it is understandable that his remarks on manuscripts carry a lot of weight. Under these circumstances I have known of instances in which a department head (i) insisted on changing the title and thus minimized the article's legitimate claim to consideration; (ii) sat on the article; (iii) penciled his name on the title page and thus became co-author.

RICHARD F. SHAW

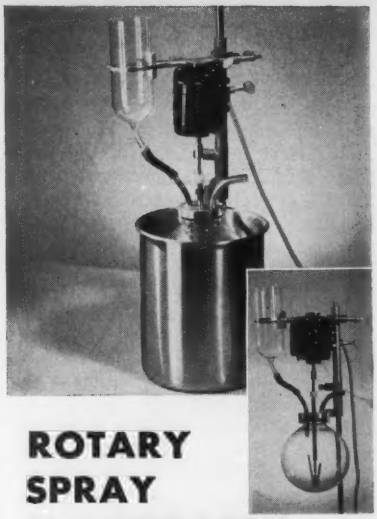
Department of Preventive Medicine, University of Virginia School of Medicine, Charlottesville

Radiation from Solar Flares

We have read with some interest the article in *Science* [**133**, 312 (3 Feb. 1961)] entitled, "Limitations on space flights due to cosmic radiations," by Howard J. Curtis. The work discussed reported on the effect of concentrated doses, and the results represent an important finding, for they completely remove any doubt which may have remained about the ability of the galactic cosmic rays to cause radiation problems in space.

However, we are somewhat disturbed that an article on an important topic which is in a journal widely read by biologists should have omitted a discussion of the potentially most troublesome source of radiation. We refer to the frequent storms of solar cosmic rays originating in large flares.

These storms have been widely discussed at meetings of the American Physical Society and the American Geophysical Union. The literature abounds with papers on the subject, many of which point out the very serious radiation hazard to space flight outside the earth's magnetic field or in the polar regions. We are quite certain that every technical group considering space flight is well aware of this problem and has gone to considerable effort to examine the difficult problem of shielding against the solar cosmic rays. We have in our files many reports on the subject of shielding, as well as records of conferences with the space agency, the Radiation Research Society, the Air Force medical groups, and other organizations. All of the authors quoted in Curtis's article, we are sure, are also familiar with the solar cosmic rays and their potentially dangerous effect from the standpoint of radiation.



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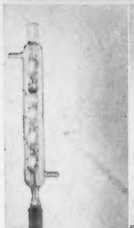

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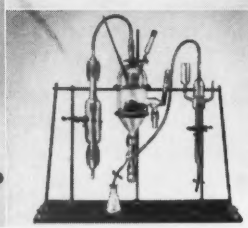

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JOHN R. WINCKLER
EDWARD P. NEY
School of Physics, University of Minnesota Institute of Technology, Minneapolis

Winckler and Ney are quite correct in indicating that the solar-flare radiation is an important aspect of the radiation hazards of space flight. At the time my article was written, nearly a year ago, there had not been enough measurements in space for us to be able to assess the biological hazard, so I discussed the flares as a perturbation on the radiation belts. Now we know, largely through the excellent work of Winckler and Ney, that they constitute an additional hazard which will be very troublesome for some types of space travel. In such an active field, a review article may be out of date by the time it is published.

HOWARD J. CURTIS
Department of Biology, Brookhaven National Laboratory, Upton, New York

Meeting of Zoologists

A most unfortunate impression of the recent annual meeting of the American Society of Zoologists is created by your story [*Science* **133**, 89 (13 Jan. 1961)]. Although few in number, those present did devote much of the meeting to matters of professional concern to the members of the society, and it was only when adjournment appeared imminent that there was introduced the statement featured in your story. The subsequent discussion was hurried, and the action of the society on the many amendments and modifications of the statement you quote [recommending government implementation of programs for research and training in the

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field of birth control] was extremely confused. My own recollection of our last action on this matter before the conclusion of the meeting leads me to consider your story seriously inaccurate.

In the course of the discussion there was raised a question of the ability of this small group to consider this matter within the limitations imposed by the new constitution of the society. Without a formal ruling on this question, the matter was put to a vote. Of approximately 60 members present, two or three more than 30 favored the proposal, and two or three less than 30 opposed. Regret at the narrow margin of voting was expressed by several speakers, including, I think, those who had introduced and seconded the original proposal. A number of modifications and amendments were then introduced and discussed. Finally, it was moved and seconded that the proposal should be submitted for rewriting to a committee to be appointed by the president of the society. This motion was passed by a clear vote, and the meeting was adjourned. It appeared to me, at that time, that such action required the submission of the rewritten proposal to a subsequent meeting, after proper consideration of the possible constitutional requirement for the submission of the question to the written vote of the entire membership.

In contrast, the action on Prosser's resolution of opposition to Senate Bill 3570 [on regulating the use of laboratory animals] was definite and strong. It was apparent to all that such legislation would present a serious threat to the professional activities of the members of the society, and that such a direct attack upon the chief concern of the society, the advancement of biological research, required the strongest action and fullest energies of the society. Such concerted action was inhibited by the second resolution, as was apparent to nearly half of those present. That this expectation has been fulfilled could scarcely have been demonstrated more effectively than it was by your story. Beyond the short introductory paragraph, you devote 36 lines to the proposal of divided interest and only 19 lines to opposition to a major threat to biology—a ratio of nearly 2 to 1 in the wrong direction.

I hope that those concerned about Senate Bill 3570 are exaggerating the danger. But discussions among people not concerned with biological research do not support that hope. Indeed, such conversations suggest that even Prosser and Wilbur, who have been particularly active in this campaign, may be underestimating the public support for this pernicious proposal. If such be the case, it appears particularly unfortunate that we should divide whatever slight influence may be exerted by the society and by the AAAS.

It is hoped that your future treatment of the actions of associated societies may reflect more accurately the intensity of the concern of the membership for proposals before the legislatures. Surely, no member of Senate or House could be blamed for concluding, after reading your report, that the American Society of Zoologists is only mildly interested in Senate Bill 3570. To me, at least, this did not appear to be the sense of the meeting.

PAUL FOLEY NACE
McMaster University, Hamilton, Canada

As retiring secretary of the American Society of Zoologists, I wish to comment briefly on Nace's assertion that the society acted hastily, confusedly, and probably illegally in passing a resolution urging governmental support of research and the training of medical personnel in the field of birth control while acting at the same time in a strong, definite, and admirable manner in passing a resolution against Senate Bill 3570, the so-called Cooper bill to regulate the use of laboratory animals. I would also like to comment on the charges that *Science* was "seri-

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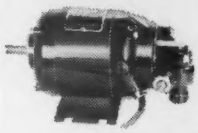

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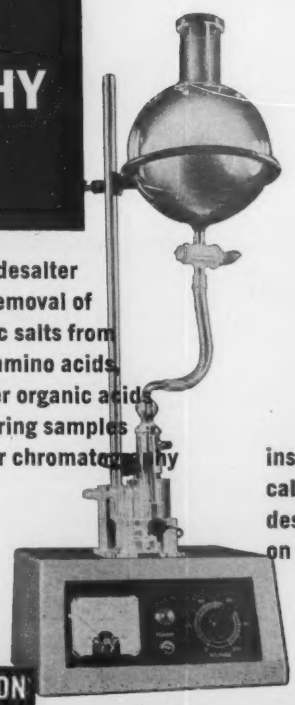

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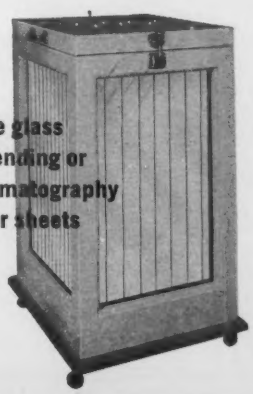

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ously inaccurate" in reporting the annual meeting at which these resolutions were passed, and that it showed gross bias in devoting 36 lines to birth control and only 19 to animals for research.

As to the facts of the reporting, there hardly is any room for inaccuracy in the *Science* account since it consists merely of a very short paragraph stating that the two resolutions were passed and then quotes each of the resolutions. The reason why 36 lines are devoted to birth control and 19 to animals for research is very simple: the birth control resolution required 36 lines to print, the animals for research resolution, 19 lines. Everyone admits that both resolutions passed and that well over a quorum was present, even under the new constitution, which more than doubled the number required.

I have reread both the new and the old constitution and can find no suggestion, much less requirement, that motions passed by the members at the annual meeting should be sent to absent members for a mail vote.

How much confusion there really was is a debatable point. A motion was offered favoring governmental support for research and medical training in the field of birth control. Only two or three people spoke against it. I myself, perhaps unfortunately, then urged an amendment, but the majority felt it weakened the resolution, which passed in its original form, 39 to 25. Thus 61 percent favored the resolution in its "strong" form. Presidents of great nations more than once have been carried into office on far slimmer margins. Nevertheless, the matter was still further discussed, and it was finally agreed, almost, but not quite, unanimously to accept the motion but with the provision that it should be reworded by a committee before publication. This was done. I was appointed to the committee myself.

Of course it is possible to argue that the customarily small number of people who show up at annual business meetings is not a representative sample. However, there are good reasons, based on past experience, for believing that, at least in the American Society of Zoologists, the members who attend are, in fact, reasonably representative.

It is also possible to argue that to advocate research and free access to scientific knowledge in the field of birth control is wrong because a scientific organization should remain morally uncommitted. This is clearly not Nace's view, because he strongly favors society action against Senate Bill 3570.

GAIRDNER B. MOMENT

American Society of Zoologists, Goucher College, Baltimore, Maryland

Institutions and Scholars

The article "Personality and scholarship" [*Science* **133**, 362 (10 Feb. 1961)] by Paul Heist, T. R. McConnell, Frank Matsler, and Phoebe Williams, of the staff of the Center for the Study of Higher Education, University of California, Berkeley (except for Matsler, who is at Humboldt State College), contains the following incorrect statement (p. 363, col. 2): "The institutions are listed in the order of the Knapp and Greenbaum indices of productivity. It may be noted that about 70 percent of the 216 male students attended the ten most productive institutions." [As stated by the authors, the Knapp and Greenbaum index of productivity was the "number of students per thousand graduates from 1946 to 1951 who later received either (i) Ph.D. degrees, (ii) university fellowships, (iii) government fellowships, or (iv) private foundation fellowships exceeding $400 per year."] Table 1 in the article is not arranged in the "order of the Knapp and Greenbaum indices of productivity," as stated. It is arranged according to the ratio of

Table 1. Institutions listed according to production of scholars, as described in the definition of the Knapp and Greenbaum indices of productivity.

School	Scholars per 1000 graduates (*N*)
Males	
Swarthmore	61
Reed	53
University of Chicago	48
Oberlin	40
Haverford	40
California Institute of Technology	38
Carleton	35
Princeton	32
Antioch	32
Harvard	27
Yale	27
Queens	26
Grinnell	24
Wesleyan	22
Kenyon	22
Johns Hopkins	21
Massachusetts Institute of Technology	21
University of the South	20
Knox College	20
Cornell	20
Cooper Union	18
Beloit	18
Columbia	18
Pomona	17
Wooster	17
Augustana	17
DePauw	17
Females	
Bryn Mawr	40
Barnard	26
Radcliffe	20
Vassar	17
Cornell	16
University of Pennsylvania	16
McMurray	12
Mt. Holyoke	12
Smith	11
Sienna Heights	11
University of Chicago	11

National Merit Scholarship students per thousand students enrolled. The actual "order of the Knapp and Greenbaum indices of productivity" [R. H. Knapp and J. J. Greenbaum, *The Younger American Scholar* (Univ. of Chicago Press, Chicago, 1953), pp. 16, 70] is as shown here in Table 1.

Although in general there is a high degree of correlation between the two listings with respect to the schools included, comparison of the two tables will show that there are gross differences between the two in the rankings for individual schools, as would be expected. Moreover, a few of the schools that appear in the listing given here do not appear at all in Table 1 of the article—for example, Antioch, Queens, Kenyon, Cooper Union, Beloit, and Wooster for males and McMurray and Sienna Heights for females. On the other hand, the following schools listed in Table 1 of the article were not among the first 27 for males in the Knapp and Greenbaum index of productivity: Amherst, Williams, Brown, University of Pennsylvania, and Dartmouth. Swarthmore and Grinnell were not among the first 11 for females. It is obvious that this is due to the meager statistical significance of many of the data in Table 1 of the article. More than half of the schools listed in that table had four or fewer National Merit Scholarship students. The table should have indicated that the data were for one year only (1956), and that many of the individual listings were not statistically significant. (Or, obviously, data that were not statistically significant could have been left out, or the table could have included the eight schools mentioned above that were in the Knapp and Greenbaum indices for productivity but did not happen to have any National Merit Scholarship students in 1956.)

There is a critical lack of published information that can give an indication of the comparative qualities of various schools; it is desirable to have information published that can be used to help fill this need. But precisely because of the intensity of interest in this kind of information, because of the use to which any data such as these will be put, and because of the sensitivity of any such ranking as this (involving both productivity and ratio of National Merit Scholarship students), it was incumbent upon Heist *et al.* to make every effort to forestall inaccurate interpretations. In this instance, without knowledge of the Knapp and Greenbaum book, it would be very difficult for the average reader to obtain an accurate impression.

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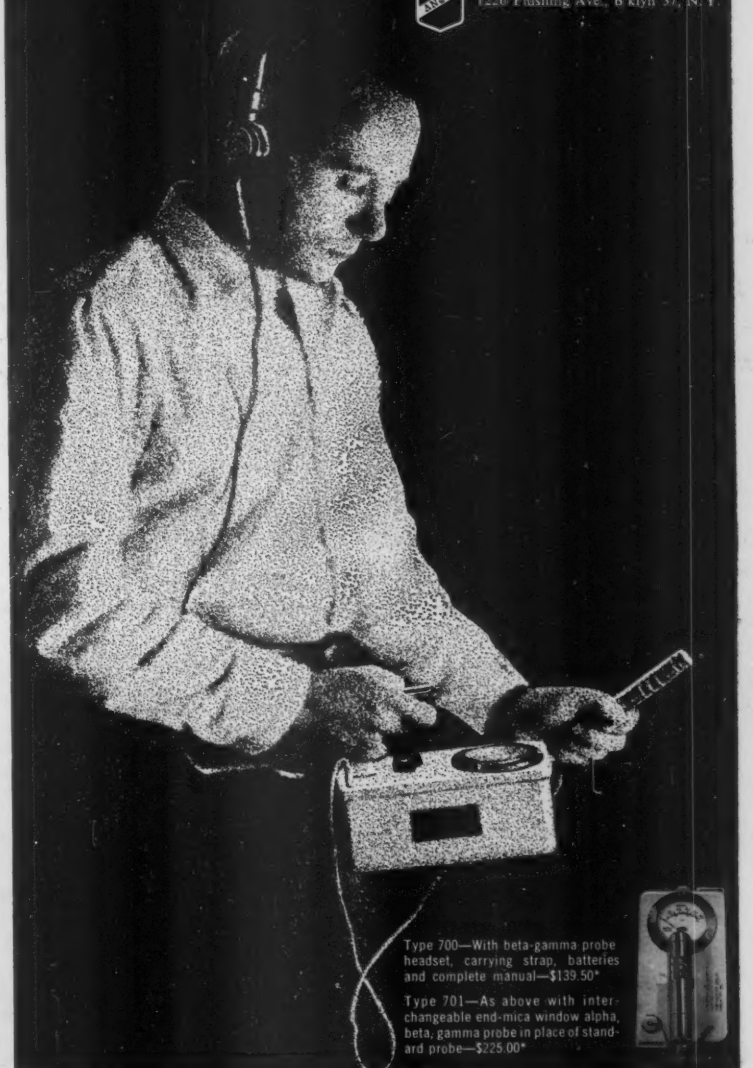

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We want to thank Robert T. Jordan for calling attention to the following incorrect statement in our article: "The institutions are listed in the order of the Knapp and Greenbaum indices of productivity." The title of our Table 1, however, is correct: "Distribution of male and female National Merit Scholarship students among institutions ranked high in the production of scientists and scholars." This table head indicates the basis for ordering the institutions.

The sentence immediately following the incorrect statement is accurate as it stands; the "70 percent of the 216 male students" refers to the number of the ten *most* productive institutions (California Institute of Technology excluded) listed in the Knapp and Greenbaum volume.

Needless to say, we would like to avoid giving the reader a basis for inaccurate interpretation, and we believe that the article does not imply that there are differences in quality among the institutions listed in Table 1. We avoided making any qualitative comparisons among these schools, since our purpose was simply to present differences between the students who entered a group of more productive institutions and the students who entered a group of less productive institutions.

There would be no point in computing the statistical significance of the differences in numbers or proportions of National Merit Scholarship students attending the institutions listed in our Table 1. This would obviously be irrelevant.

For the purpose of simplicity, we included in our Table 1 only those of the 50 highest-ranking institutions in the Knapp-Greenbaum list in which one or more of the 1956 National Merit Scholarship students happened to be enrolled. We see no reason why a careful reader would find any implication concerning the relative quality of institutions in this table.

Knapp and Greenbaum did not include Swarthmore and Grinnell in their final list for females because the two institutions did not have 400 graduates over the period of the study. We included these two colleges in our Table 1 for the following reasons: (i) the number of graduates from these institutions approached the Knapp-Greenbaum criterion figure of 400 (397 for Swarthmore and 372 for Grinnell) and far exceeded the figure for most of the other institutions; (ii) the Knapp-Greenbaum indices of the number of female scholars per 1000 graduates [Knapp and Greenbaum, *The Younger American Scholar* (1953), appendix 1, p. 103] placed these two institutions second and third, respectively, following only Bryn Mawr in order of rank; (iii) both institutions ranked high on the indices for male graduates.

We believe that the text made it clear that the data were for one year only, since it was stated that "the sample . . . consisted of all the winners and a 10-percent sample of those who received certificates of merit (the near-winners) from the National Merit Scholarship Corporation in the spring of 1956."

PAUL A. HEIST
T. R. MCCONNELL
Center for the Study of Higher Education, University of California, Berkeley

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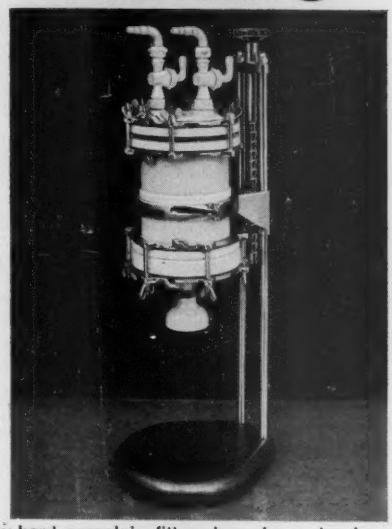

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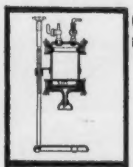

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N-CBZ-VALYL GLYCINE ETHYL ESTER
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L-CYSTINE HYDROXAMATE
L-GLUTAMIC ACID HYDROXAMATE
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GLYCYL-L-PROLINE
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p-TOSYL-L-LEUCINE BENZYL ESTER
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lever-pressing conditioning, the occurrences of the two conditioned responses being positively correlated."

The method of combining the salivary (type 1) with the motor (type 2) conditioned response used by Shapiro was first described by Konorski and Miller as early as 1930, in French, under the title "Méthode d'examen de l'analysateur moteur par les réactions salivo-motrices" (*2*). Since that time a great number of experimental studies have been published in which this method was used in various experimental conditions. Although extensive monographs dealing with our prewar studies of this subject were published only in Polish (*3*) and Russian (*4*), references to these and other papers, as well as the general description of the facts obtained, were presented not only in my English monograph concerning conditioning (*5*) but also in various American papers and monographs by Razran (*6*), Hilgard and Marquis (*7*), and others. After the war all papers of our laboratory were published in English in *Acta Biologiae Experimentalis*, a journal easily available in America. The method is also used in several Russian laboratories.

It is really regrettable that facts which are generally known by those concerned in the given subject are published among the papers reporting new scientific achievements.

J. KONORSKI
Department of Neurophysiology, Institute of Experimental Biology, Warsaw, Poland

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1. M. M. Shapiro, *Science* **132**, 619 (1960).
2. J. Konorski and S. Miller, *Compt. rend. soc. biol.* **104**, 909 (1931).
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5. J. Konorski, *Conditioned Reflexes and Neuron Organization* (Cambridge Univ. Press, London, England, 1948), pp. 267 ff.
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Konorski has raised the question of the extent to which my report in *Science* was a new contribution to the study of the relationship between operant and respondent conditioning. Unfortunately, particularly in psychology, even minor differences in experimental procedure seem to make quite a difference both in the results obtained and in their interpretation. There are major differences between the procedures used by Konorski in his studies and those used in my series, of which the report to which we are referring was the first publication. (i) In the work described by Konorski (*1*), a leg-lifting

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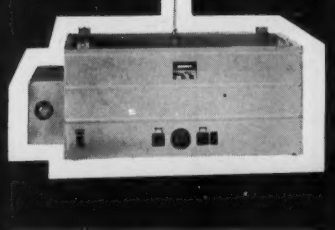

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response was conditioned by following the passive lifting of the leg with food. The experimenter, by mechanical means, produced the response in the initial training trials. In the experiment which I reported, lever pressing was conditioned by following its operant occurrence with food. (ii) Konorski's animals were restrained in a stand. My animals were in an experimental space with freedom of movement. (iii) The relationship between operant and respondent behavior was studied with regard to a specific schedule of reinforcement—fixed interval. It is conceivable that the nature of the relationship will be found to be a function of the different reinforcement schedules—that is, "facts which are generally known by those concerned in the given subject" may not be general facts.

I was pleased to find that Konorski had read my paper in *Science*, or the reprint which was sent to him unsolicited. I have always found the published reports of Konorski's experiments most interesting and was grateful for the opportunity to discuss them with him personally on two different occasions, once in Baltimore, Maryland, and once in Bloomington, Indiana.

MARTIN M. SHAPIRO
Department of Psychology,
University of Houston, Houston, Texas

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1. J. Konorski, *Conditioned Reflexes and Neuron Organization* (Cambridge Univ. Press, London, England, 1948). pp. 2-232.

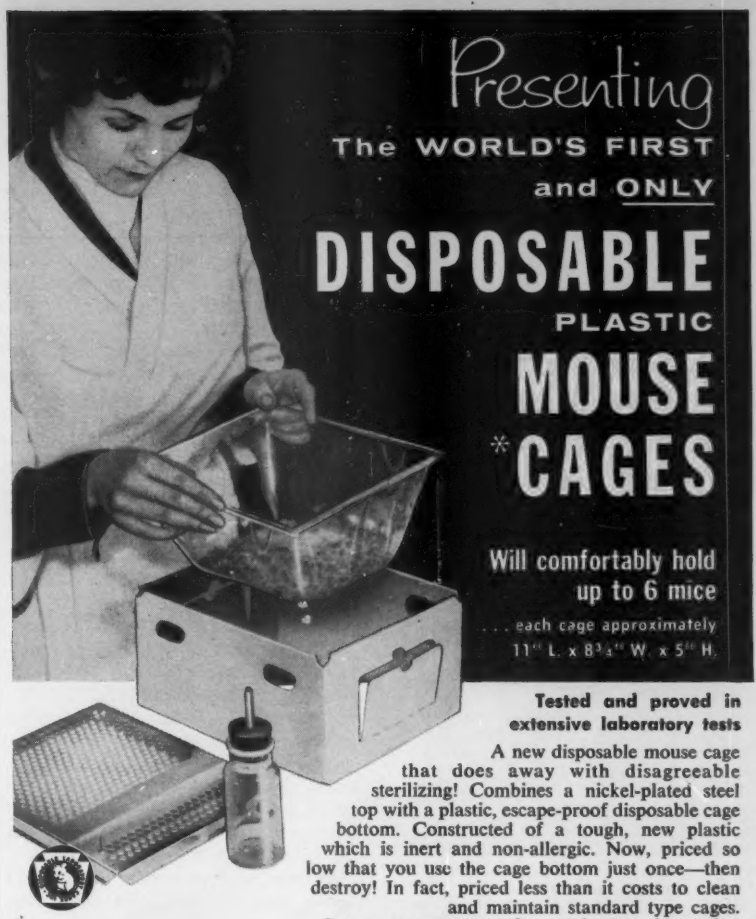
Wildlife Ranges in Alaska

I have tardily read a news note [*Science* **132**, 1878 (1960)] dealing with Alaskan wildlife ranges. It is unfortunate that so respected a publication should inadvertently be a tool for propagating misinformation concerning these national wildlife ranges.

First, I would like to observe that Canada has expressed only opposition to the establishment of comparable ranges in the Yukon Territory adjacent to the Arctic Wildlife Range of Alaska.

Second, I would like to point out that the ranges are not needed for the protection of "Arctic caribou, grizzly bears, Dall sheep, moose, and marine mammals, whose numbers are dwindling," as reported in the published news note. Many of these species are probably as abundant as ever before in the history of white man's occupancy of Alaska.

Alaska's caribou, sheep, and moose populations are all at high levels of abundance, and even the most intensively hunted populations around human population centers are being maintained or increased under the state's



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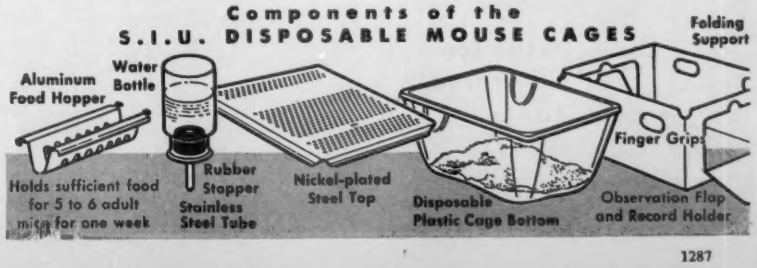

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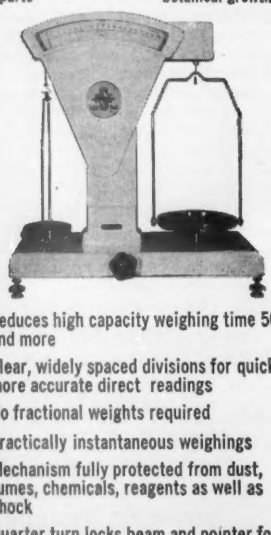

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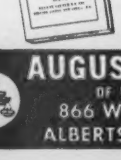

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management program. We recognize that grizzly bears are not compatible with land development by human beings, but even these animals are certain to maintain their numbers over most of Alaska with the protection and management being afforded them by the state.

The question as to whether or not there was justification for establishing additional, enormous wildlife ranges in Alaska does not, therefore, hinge on the welfare and continued abundance of certain species of game animals, but is rather tied in much more closely with the issue of whether federal or state control of huge parcels of land is the more desirable.

Federal control of vast areas in Alaska precludes implementation of Section 4, Article 8 of the Constitution of the State of Alaska, which reads: "Fish, forests, wildlife, grasslands, and all other replenishable resources belonging to the State, shall be utilized, developed and maintained on the sustained yield principle, subject to preference among beneficial uses."

It is the contention of many Alaskans that absentee authorities, which so long directed the destiny of Alaska as a territory, are less apt to provide the wisest possible management of the state's resources. It is, furthermore, certain that Alaskans above all others cherish and wish to preserve their wild and renewable resources, including the wilderness aspect of Alaska.

In view of these considerations I suggest that the pros and cons concerning establishment of these new wildlife ranges in Alaska are somewhat complex, and that more is involved than the protection of certain animals, as implied in your news note.

C. L. ANDERSON

Alaska Department of Fish and Game, Juneau

Quantum Mechanics and Freedom

The article by S. S. Kety, "A biologist examines the mind and behavior" [*Science* **132**, 1861 (1960)], points out correctly that in order to grasp a biological problem the investigator has to understand the necessity of using various approaches in both the methodological and the conceptual sense, because a biological structure is a composite of different levels of organization. Kety errs, however, when he brushes away, as irrelevant to the problems of mind and behavior, the principle of indeterminacy of quantum mechanics and the acausal character of the elementary quantum processes, by assumming that the source of this principle and of the acausality of elementary quantum processes is the clumsiness of our instru-

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Examine these relationships when variations in findings are difficult to explain

There are a number of factors which alert investigators must constantly scrutinize and evaluate if biological experimentation is to result in maximum productivity.

One of the most important of these is the relationship of one factor to another. For should the reaction of these relationships be overlooked, variations in experimental results would be hard to trace.

What are these relationships? Some of the more basic ones are the relationship of nutritional requirements to: body surface area; energy-amino acid content of the diet; food intake. And within the nutrients themselves, many other relationships exist. Relationships such as those indicated by an optimum balance between essential amino acids; the effect of change in the calcium-phosphorus ratio; and the sparing effect of niacin on the tryptophane requirement.

Some relationships are more complex than others. For example, one of the most critical relationships which the investigator should consider is the relationship of physiologic status and nutritional deficiencies. This relationship is indicated when nutritional abnormality results in a diseased state. Often this presents a perplexing problem because systemic disease unrelated to nutrition may precipitate a nutritional deficiency even though normally adequate intake of nutrients is maintained. The use of diets improperly balanced and controlled (from a quality or manufacturing viewpoint) could cause even further variations in findings.

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ments, which has made it impossible for us to determine, simultaneously, the position and motion of every particle. ("It would seem," he says, "that the concepts of freedom and purpose in the universe should be based upon nobler stuff than the clumsiness of our instruments.")

The point made by the quantum mechanics is that the inability to establish, simultaneously, precise measurements of the position and motion of an elementary particle is due to the inherent acausal "freedom" of this particle. As expressed by P. A. M. Dirac [*The Principles of Quantum Mechanics* (Oxford Univ. Press, New York, new ed., 1935), p. 10]: "When an observation is made on any atomic system which has been prepared in a given way and is thus in a given state, the result will not in general be determinate, i.e., if the experiment be repeated several times under identical conditions several different results may be obtained." Consequently, if certain mental processes are elicited by quantum processes on the molecular level of observation, which seems quite possible, the quantum mechanical "freedom" as highly relevant to these processes is a hypothesis not to be minimized in our concepts of the problems of mind. I am referring, in this connection, to the book by Pascual Jordan, *Die Physik und das Geheimniss des organischen Lebens* (Vieweg and Sohn, Brunswick, Germany, 1948).

SILVIO FIALA
Department of Pathology, Columbia University, New York, New York

I am grateful to Fiala for pointing out what could have been interpreted as a rather cavalier dismissal of a controversy which has rocked modern physics for the past three decades.

While the Heisenberg principle of indeterminism may originally have been only a simple recognition of instrumental interference, it quickly became elaborated by Bohr and by Heisenberg himself into a doctrine of the inherent unpredictability and, in fact, acausality in the behavior of the elementary subatomic particles. This inference of the "Copenhagen school of physics" has received wide acceptance by quantum physicists, although it would not be correct to imply that it has achieved universal acceptance. In fact, de Broglie, Planck, and Einstein remained unconvinced of its validity.

I should like to point out, however, that those who accept that doctrine and wish to use it as a scientific basis for a belief in human freedom should be willing to assume a difficult if not impossible task. Since a single neuron is composed of thousands of billions of such elementary particles and a single

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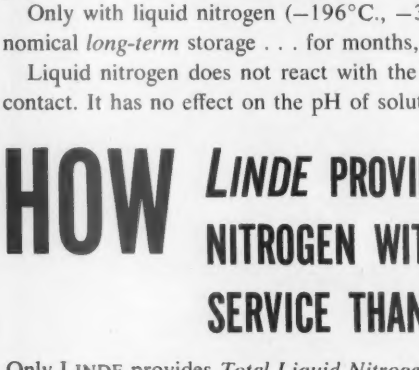

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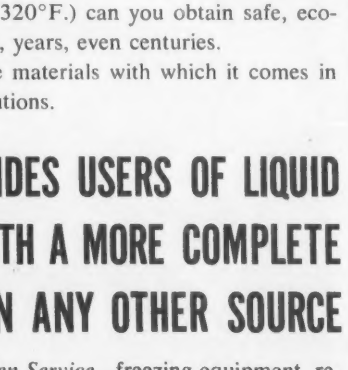

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voluntary act requires the approximately simultaneous action of at least several thousand neurons in concert, it will be necessary for them to explain what is difficult for me to imagine—the means by which sufficient order, let alone purpose, emerges from the random behavior of these countless congeries of particles, especially since the very concept of acausality brooks no possible interaction in this respect between the particles. It may be possible to imagine a means whereby certain particles may be given the possibility of dictating the behavior of the whole. Even by this great stretch of the imagination, however, what would result would not be "free will," or freedom of the individual or even of a single neuron, but a response which was completely random, acausal, and nonpurposive.

I should like to amend my previous statement, quoted by Fiala, to read: "It would seem that the concepts of freedom and purpose in the universe should be based upon nobler stuff than the clumsiness of our instruments *or the acasual, random noise of the subatomic particles.*"

SEYMOUR S. KETY
Laboratory of Clinical Science, National Institute of Mental Health, Bethesda, Maryland

I hope that Kety's article will be read widely, especially by college presidents, college division or department chairmen, and state and federal civil service executives. However, as one who has grubbed hard and long most of his professional life trying to modify human behavior through the techniques of social work and group psychotherapy, I am afraid that the fond wish behind such sentiments has little chance of being realized. I think that Kety's eloquent plea for flexibility and increased communication among the various disciplines, or even for checking the proliferation of specializations which continues to sweep the academic and scientific communities, will only be heard by those like myself who are already convinced.

Besides the over-all excellence of his essay I was especially delighted by his charming and sly parable, "The true nature of a book." Although it falters somewhat when it touches on the last group, the behavioral scientists ["Finally, the book is brought in desperation to the psychoanalyst in the hope that he will be able to read it. That he does not do precisely, but instead asks the author to select portions and read them while he listens. . . ." (What author?)], its over-all impact is not inconsiderable.

Kety suggests that psychoanalysts, despite their biases—or even because

of them—or their lack of scientific discipline, may have a great deal to offer. This is all to the good. But why stop at psychoanalysts? How about social workers, who are also in the day-to-day business of trying to understand and modify behavior? Although their terminology is not as exotic as that of the analysts, I can assure you that they are just as biased, and furthermore, they outnumber psychoanalysts about 50 to 1.

And finally, let us not forget the poets. Perhaps in the last analysis it may very well be the rare insight possessed only by some future poet which will unlock a few of the mysteries of human behavior.

JOSEPH ANDRIOLA
Department of Mental Hygiene, Atascadero State Hospital, Atascadero, California

Palestine Refugee Problem

Wendell Cleland, in his review of Lengyel's *The Changing Middle East* [*Science* **132**, 614 (1960)] cites Lengyel's failure to mention the assassination of Count Bernadotte as an example of pro-Israel bias. The assassination of Count Bernadotte, for which the small and dissident Stern group was held responsible, was abhorred and condemned by the government and people of Israel. The group was forcibly disbanded immediately afterwards. The military action of the Irgun Zvi Leumi (Etzel) at the village of Dir Yassin, near Jerusalem, took place before Israel existed as a state. Both of these terrorist groups during the British mandate refused to accept the discipline or the democratic will of the Jewish community of Palestine, expressed through its national council and defense group, Haganah. To hold Israel responsible for these two actions, condemned by the Jewish authorities, population, and defense forces, is certainly unjust. Moreover, this action at Dir Yassin was the only action of its kind conducted by any Jewish group, dissident or otherwise; hence, the expression "the massacres of Arab villagers which created the refugee problem" is a distortion.

The fact is that the Dir Yassin incident did not create the Palestine Arab refugee problem, nor the problem of the 450,000 Jewish refugees who fled from the Arab states (a problem which Cleland significantly fails to mention). Both of these refugee problems were the direct outcome of the Arab invasion of Palestine in 1947–1948 in violation of the United Nations Partition Decision of 29 November 1947. Had this invasion not taken place, had the Arab command not issued instructions to the

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Palestine Arabs to evacuate the designated battlefield, and had these Arabs listened to Jewish appeals to stay put, as those who chose to remain and are today free citizens of Israel did, no Palestine Arab refugee problem would today exist.

Cleland's phrase "or the ignoring [by Israel] of United Nations' resolutions looking toward a settlement" make strange reading in the light of the fact that the two basic resolutions, the U.N. Partition Decision and the Armistice Agreements between Israel and each of the Arab states, entered into under United Nations auspices, have been violated by the Arabs since 1947. No United Nations resolution has been violated by Israel. The Armistice Agreements were designed by their architects as a bridge to peace, and they have the inbuilt provision that the two sides will sit together and amicably resolve still outstanding problems. The Arabs have refused to sit with any Israel delegation to discuss these problems but instead have resorted to maritime blockade, economic boycott, infiltration, and armed attack and have waged war by other means. In the United Nations resolutions, return of the Palestine Arab refugees is made conditional on the willingness of the Arabs to live in peace and on the practicability of the return. The Arabs have expressed the opposite of such willingness and by their belligerence have made their return impractical. Not Israel but the Arab states have violated even this resolution. The very bias that Cleland claims to find exhibited by the author of *The Changing Middle East* is, unhappily, displayed by his own comments.

YAAKOV MORRIS
Department of Research and Publications, Israel Office of Information, New York, New York

Morris's repudiation of the actions of the Stern Gang and the Irgun Zvi Leumi are welcome. But the general opinion is that the State of Israel did not take sufficient action to redeem the bad situation these bandits caused. On the contrary, Israel has accepted every advantage she got from their criminal actions.

As for why the Arabs evacuated their homes, the situation certainly was chaotic, but there is little doubt, if any, that the evacuation was sparked by the Dir Yassin massacre, which occurred five weeks before any of the Arab states sent their forces into Palestine—a move made to preserve order in the vacuum created by the British withdrawal on 14 May 1948.

As to Morris's statement that "no United Nations resolution has been violated by Israel," the facts are quite the opposite. Three of the United Nations resolutions of 1947 and 1948 are

still being ignored, in whole or in part.

1) The boundaries set in the original resolution of 1947, which would require Israel to return to the Arabs 22 percent of her present territory occupied during the fighting, but of which Israel has declared she will yield absolutely none. The implication that the Armistice Agreements of 1949 confirm these boundaries is quite unjustified, as the Egypt-Israel Agreement, article V, paragraph 2, reads, "The Armistice Demarcation Line is not to be construed in any sense as a political or territorial boundary, and is delineated without prejudice to rights, claims and positions of either Party to the Armistice as regards ultimate settlement of the Palestine question." If Israel holds that the armistice boundaries are unchangeable, then the armistice continues, and that the war is only in suspension, with no peace in sight.

2) The right of the refugees to opt whether they will return to their homes and property or accept compensation. The option has been denied, except in a limited number of cases where a choice was allowed in order to reunite families. Morris's statement that the Arabs "by their belligerence have made their return impractical" would seem to be related to the haste of the government of Israel to bring in a million Jews from other countries since 1949 in order to occupy the Arab lands and make the return to the owners impossible.

3) The international status of Jerusalem. Israel has totally ignored this resolution, has incorporated parts of this area in her state, and has declared Jerusalem her capital. This defiant action on the part of Israel has not been accepted by the United States Government and by certain other members of the United Nations, who refuse to acknowledge Jerusalem as the capital and still maintain their embassies at Tel Aviv. Any official negotiations between the U.S. embassy and the Israeli Foreign Office have to take place in Tel Aviv, or outside the international zone.

The other occupant of the Jerusalem zone is Jordan, and Jordan will not agree to negotiate evacuation as long as Israel publicly proclaims her right to Jerusalem, contrary to the United Nations resolution.

There is no question that the situation in Palestine is becoming more and more complicated, and it seems to me that the burden of proof as to rights there lies distinctly on those whose invasion of the country, previously a peaceful land, brought on all the troubles.

W. WENDELL CLELAND
American University,
Washington, D.C.

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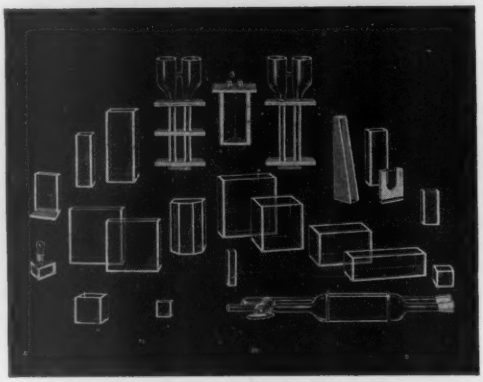

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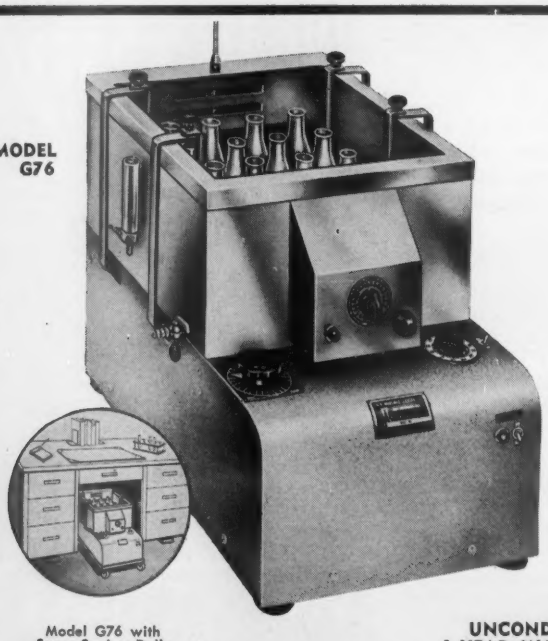

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■ COLOR-MIXTURE COMPUTER displays on an oscilloscope a series of dots representing a spectrophotometric curve. Each dot represents one of 10 or 16 equations (depending on the model), which are to be solved simultaneously. Each equation represents the sum of the contributions to light absorption made by each dyestuff or pigment at a particular wavelength and at the concentration set into the computer. If the sum of these contributions is equal to the light absorption of the standard to be matched, the dot representing the equation appears at the zero line of the oscilloscope. When all dots appear at the zero line, all equations have been solved and the reflectance of the proposed match equals that of the standard at all wavelengths. The concentrations of the chosen colorants required are read from dials that have been adjusted to achieve the solution.

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To make a color match, the reflectance of the standard is measured at 10 or 16 wavelengths, and the reflectance values are changed to values of the appropriate absorption function by means of tables. These values are set on the computer dials. The plug-in units representing colorants to be used are inserted. The concentration dials are adjusted until a straight line appears on the oscilloscope, and finally the concentrations of colorants are read from the adjusted dials. (Davidson and Hemmendinger, Dept. Sci168, 2857 Nazareth Rd., Easton, Pa.)

■ MCLEOD-TYPE GAGE features measuring capillaries that are completely separate and that may be detached from the main body of the gage for replacement or cleaning. The separate capillaries afford two ranges of pressure measurement: 0.150 to 2 mm and 1 to 2000 μ. The capillary tube can be replaced without recalibration (see Fig. 1). (Kinney Div., New York Air Brake Co., Dept. Sci107, 3529 Washington St., Boston 30, Mass.)

■ PHYSIOLOGICAL GAS ANALYZER measures oxygen concentrations within blood vessels or other body-fluid reservoirs. The oxygen detector is an electrode; its sensing end is a tiny fleck of platinum 0.005 in. in diameter. The sensing end, encased in layers of glass and silver, is sheathed in a stainless-steel tube. The assembly fits inside a hypodermic needle of the size normally used to withdraw blood samples from a patient's veins. The electrode tip is capped with a thin polyethylene membrane that permits the passage of oxygen. Current flow at the electrode tip is proportional to the amount of oxygen present. The current is amplified to drive a meter that registers the oxygen concentration. (Beckman Instruments, Inc., Dept. Sci144, 1117 California Ave., Palo Alto, Calif.)

■ SPECTROPHOTOMETER ACCESSORY permits the measurement and recording of absorbance of multiple samples as a function of time. The instrument is said to be linear with absorbance over a range of 3 optical density units with sufficient sensitivity to permit scale expansion to 0.1 density unit full scale. The accessory is designed to be attached to such spectrophotometers as the Beckman DU, the Zeiss PMQ II, and others, without modification of the instruments. (Gilford Instrument Laboratories, Inc., Dept. Sci135, Oberlin, Ohio)

■ DIFFERENTIAL AMPLIFIER has common mode rejection of 10^8 at d-c and 10^9 at 60 cy/sec. Zero stability is said to be better than 0.05 percent of full scale for 1000 hr. Output is ± 10 volts at 200 ma into a 50-ohm load. The output can tolerate capacitive loading up to 10 μfd without going into oscillation. (Video Instruments Co., Dept. Sci160, 3002 Pennsylvania Ave., Santa Monica, Calif.)

■ PRESSURE TRANSDUCER of the flush-diaphragm, strain-gage type features extremely small size. Diameter is 0.59 in., thickness is 0.05 in., and weight is 0.5 g. Ranges are 0 to 10 lb/in.^{2}, available in absolute, gage, or differential models. Excitation is 15 volts, and output is 5 mv full scale on open circuit. Combined error due to nonlinearity and hysteresis is said to be less than ± 1.0 percent of full scale. Ambient temperature limits are $-65°$ to $+150°$F. (Statham Instruments Inc., Dept. Sci153, 12401 W. Olympics Blvd., Los Angeles 64, Calif.)

■ FLAW-DETECTOR ATTACHMENT permits an ultrasonic flaw detector to be used to measure the thickness of metals and plastics from one side, with accuracy said to be ± 0.01 in. The attachment consists of a fixed scribed vertical line and a second line on a slider movable across the face of the display cathode-ray tube. A synchronized indicator measures the amount of this motion permitting direct determination of thickness without calculation or interpolation. (Branson Instruments, Inc., Dept. Sci154, 40 Brown House Rd., Stamford, Conn.)

■ HIGH-CURRENT ADAPTER for the manufacturer's type 575 transistor curve tracer provides 200-amp collector displays, three ranges of collector supply, and 12-amp base supply. The adapter

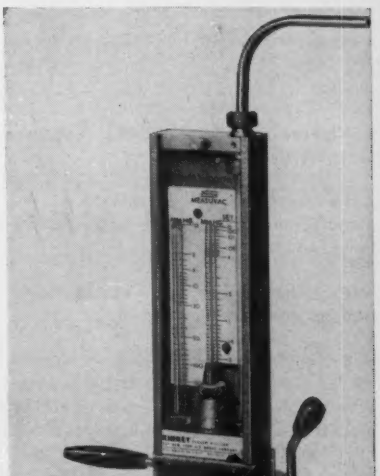

Fig. 1. McLeod-type gage.

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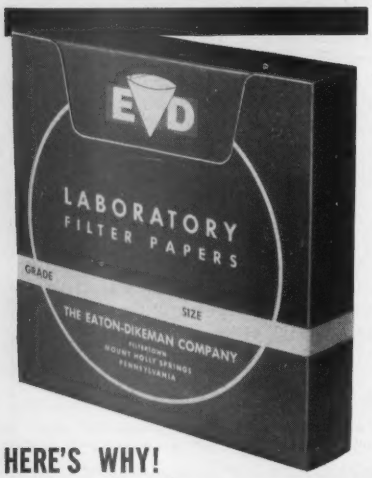

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■ TACHOMETER accomplishes digital measurement of shaft speeds by use of an integral light source, a 60-segment rotating interrupter, a photo-junction cell, and a transistor amplifier. Every 6 deg of shaft rotation results in a 2-volt pulse. The resultant square wave is fed into a digital counter with one-second base time to provide a reading of speed in revolutions per minute. Range is 0 to 25,000 rev/min; torque required is 0.05 in. oz at 3600 rev/min. (Servo-Tek Products Co., Dept. Sci147, 1086 Goffle Rd., Hawthorne, N.J.)

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■ TEMPERATURE RECORDER is a miniature thermocouple instrument that operates directly from the thermocouple without signal amplification. The instrument operates on the clamper-bar principle with a 4-sec clamping period. Pressure sensitive chart paper is used to record. Minimum temperature span is 0° to 500°F; maximum span is 0° to 2500°F. Accuracy is said to be ±2 percent of full scale. Electrical requirement is 115 volt a-c. (Assembly Products, Inc., Dept. Sci158, Chesterland, Ohio)

■ INFRARED DETECTORS of photoconductive indium antimonide peak at a wavelength of 6.8 μ. Time constant is said to be less than 1 μsec. The detectors are custom built and supplied in any configuration including arrays. (Block Associates, Inc., Dept. Sci172, 385 Putnam Ave., Cambridge, Mass.)

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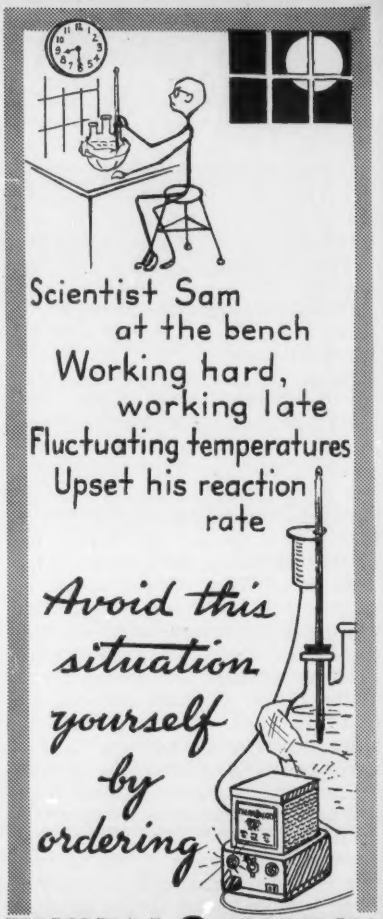

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■ PHYSIOLOGIC DATA MONITOR displays condition of the patient continuously and automatically in digital form. All parameters are also recorded on a 10-in.-wide record. Monitor-scope and wave form recorders for electrocardiograms and electroencephalograms are included. Plug-in units and transducers are available for a variety of measurements including temperature, blood pressure, heart rate, respiration rate, skin resistance, medication flow, urine output, oxygen saturation, carbon dioxide level, and others. An alarm function is actuated should any deviation from preset limits occur in the condition of the patient. Modular construction permits expansion or modification as the situation requires. (Starling Corp., Dept. Sci171, 2047 Sawtelle Blvd., Los Angeles 25, Calif.)

■ X-Y PLOTTER is a 30- by 30-in. instrument said to be accurate to ±0.05 percent of full scale and to repeat exactly from any direction. Input to the plotter may be IBM cards, punched paper tape, or keyboard. Slewing speed is 20 in./sec. The printing head contains 12 symbols, which can be selected at random. Other print sectors can be added to plot digital as well as symbol information. (Gerber Scientific Instrument Co., Dept. Sci156, Hartford, Conn.)

■ IONIZATION DETECTOR MODULE permits the modification of existing gas chromatographs to flame ionization detection. The module permits 2-in. insulation to be used between the detector burner assembly and the bulkhead which contains terminals for hydrogen, combustion air, and high-voltage lines and for the electrometer and flame igniter. Collector insulation is sapphire. The manufacturer's model 21 electrometer is normally used with the detector. (Loe Engineering Co., Dept. Sci162, 2092 N. Lincoln Ave., Altadena, Calif.)

■ ULTRAVIOLET-VISIBLE SPECTROPHOTOMETER covers the ranges 190 to 390 $m\mu$ in the ultraviolet and 350 to 750 $m\mu$ in the visible. Dispersing element is a 60-deg, fused-quartz prism. Each spectral range is recorded linearly in

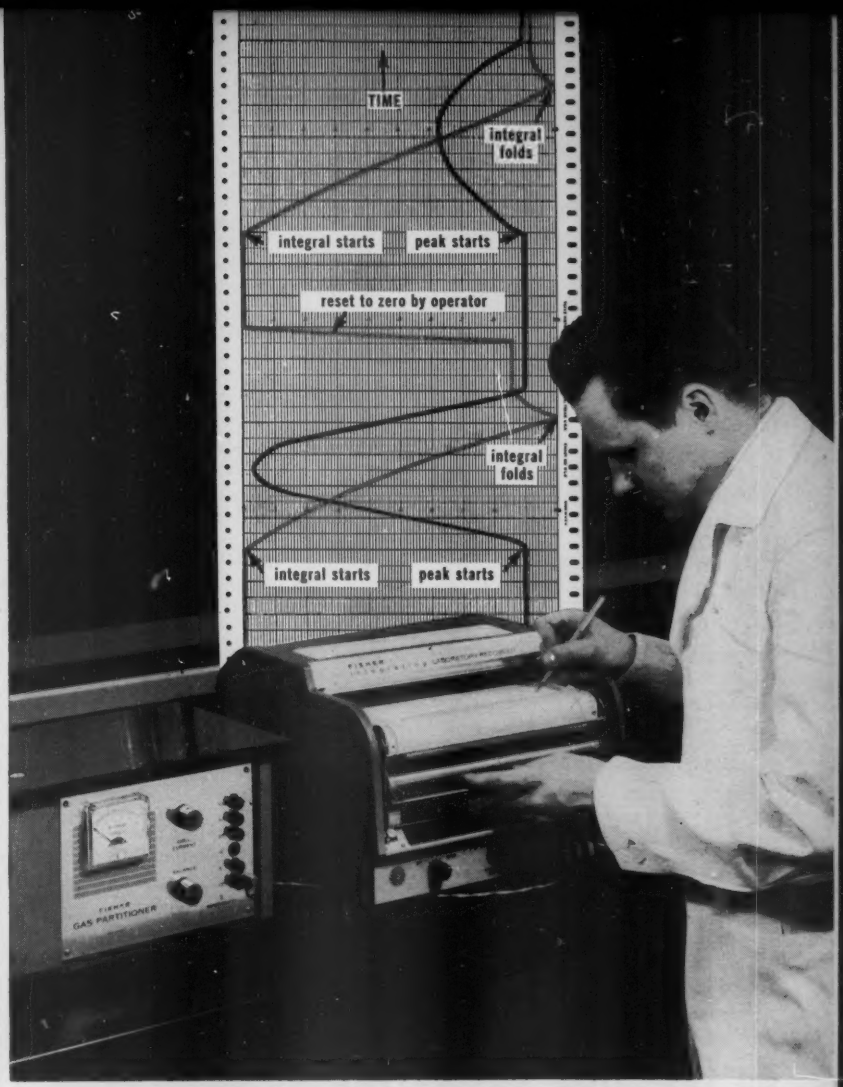

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Fig. 2. Oscilloscope trace plotter.

absorbance with a range of 0 to 1.5 units on 8.5- by 11-in. preprinted chart paper. Changeover from one range to the other is accomplished by moving the recorder drum past the end of its normal travel. Accompanying interchange of an air-cooled hydrogen lamp for ultraviolet work and a tungsten-filament source for visible work is automatic. Two scanning rates can be used, covering the spectral ranges in 2 or 8 minutes. An automatic gain control system increases spectral information in regions of heavy absorption. Detector sensitivity gain is maintained sufficiently at all times. (Perkin Elmer Corp., Dept. Sci167, Norwalk, Conn.)

■ MICROMANOMETER measures differential gas pressure with full-scale sensitivity of 0.004 in. The measuring head consists of two symmetrically arranged cavities, separated by a metal diaphragm, and two fixed electrodes. These form two capacitors that constitute part of the capacitances of two tuned circuits. Difference of capacitance of the two capacitors, caused by motion of the diaphragm, is measured and displayed on a panel meter. Several measuring heads of different ranges can be used with the same instrument. Static working pressures up to 1000 lb/in.^{2} and differential overloads up to ten times full-scale pressure can be applied without damage. The sensitive element in the measuring head is said to respond to sinusoidal pressure variations of 200 cy/sec. A switch is provided to increase the time constant of the measuring circuit to 0.3 to eliminate rapid fluctuations of measured values. Output for operation of an external recorder is included. (Gelman Instrument Co., Dept. Sci174, 106 N. Main St., Chelsea, Mich.)

■ OSCILLOSCOPE TRACE PLOTTER (Fig. 2) uses a photoelectric pickup to detect, amplify, and plot trace deflections as small as 1 mμ and is capable of time resolution in the submillimicrosecond region to the limits of the manufacturer's TW deflection tube. Designed for use with the manufacturer's high-frequency oscilloscope, the plotter can nevertheless be used with virtually any oscilloscope, according to the manufacturer. The trace on the face of the oscilloscope tube is reproduced on a prescaled piece of graph paper on an *x-y* recorder; sensibility of the trace is increased about 15 times. Settings of the *x* and *y* axes are independent of each other; magnification of the axes is arbitrarily adjustable as determined by the recorder. According to the manufacturer, no optical distortion of the image is introduced and no accuracy is lost in converting the pulse from the cathode-ray tube to electrical *x-y* analogs. A trace or a particular section of a trace may be scanned and magnified to obtain full-scale recorder drawings of deflections as small as 1 μ. Traces are reproduced in 1½ to 3 min. (Edgerton, Germeshausen & Grier, Inc., Dept. Sci94, 170 Brookline Ave., Boston, Mass.)

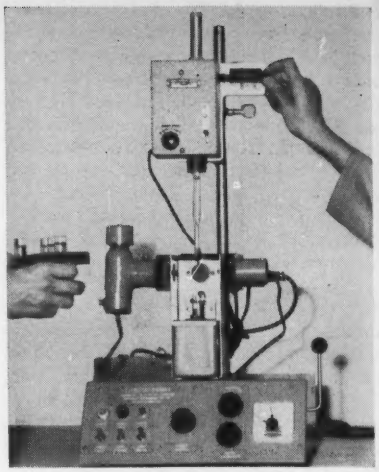

Fig. 3. Automatic titrator.

■ AUTOMATIC TITRATOR (Fig. 3) uses a motor-driven burette that dispenses titrant into a vessel placed in the path of a beam of light which passes through the unknown solution and then into a detector. The instrument automatically stops the titration and provides a digital readout of the equivalence volume of titrant when the end point, determined by color change, is reached. Data obtained may be fed to a recorder, volume data may be printed, or data can be logged on printers or computers exhibiting both sample and titrant volumes. A remote-readout digital indicator permits monitoring of the burette volume from another area. (American Instrument Co., Dept. Sci133, 8030 Georgia Ave., Silver Spring, Md.)

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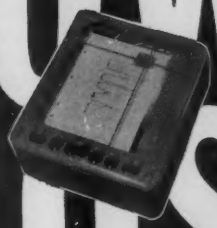

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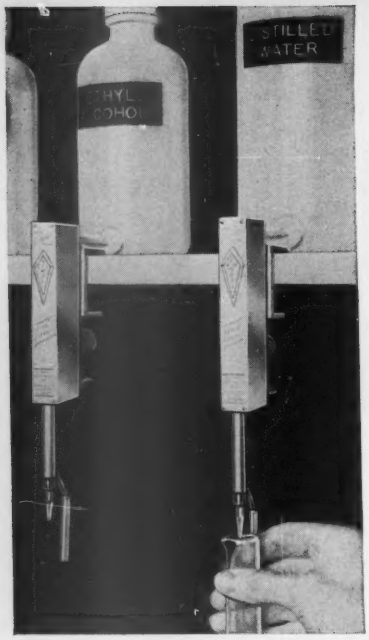

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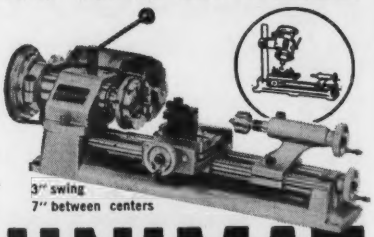

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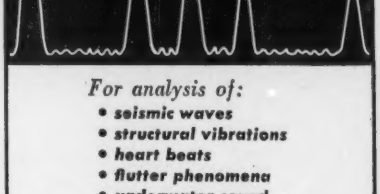

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Meetings

Forthcoming Events

May

5–7. American Soc. of Internal Medicine, Miami Beach, Fla. (G. T. Bates, 350 Post St., San Francisco 8, Calif.)

5–7. Wisconsin Acad. of Sciences, Arts, and Letters, 91st annual, Waukesha. (T. J. McLaughlin, Secretary, 2865 N. Prospect Ave., Milwaukee, Wis.)

5–8. American Psychoanalytic Assoc., Chicago, Ill. (Mrs. H. Fischer, 1 E. 57 St., New York 22)

6–7. Academy of Psychoanalysis, annual, Chicago, Ill. (J. H. Merin, 49 E. 78 St., New York 21)

6–9. Circuit Theory, 5th midwestern symp., Urbana, Ill. (M. E. Van Valkenburg, Dept. of Electrical Engineering, Univ. of Illinois, Urbana)

7–10. American Inst. of Chemical Engineers, Cleveland, Ohio. (J. F. Van Antwerpen, American Inst. of Chemical Engineers, 25 W. 45 St., New York 36)

7–11. Institute of Food Technologists, New York, N.Y. (C. S. Lawrence, 176 W. Adams St., Chicago 3, Ill.)

7–12. Medical Library Assoc., Inc., Seattle, Wash. (Miss R. J. Mann, Mayo Clinic Library, Rochester, Minn.)

7–12. Society of Motion Picture and Television Engineers, Toronto, Canada. (SMPTE, 55 W. 42 St., New York 36)

8–9. Titrimetric Methods of Analysis, symp., Cornwall, Ontario, Canada. [J. R. McCallum, Courtaulds (Canada) Ltd., Cornwall]

8–10. Aerospace Electronics Conf., 13th annual natl., Dayton, Ohio. (R. G. Stimmel, Institute of Radio Engineers, 1 E. 79 St., New York 21)

8–10. Instrument Soc. of America, Power Instrumentation Symp., 4th natl., Chicago, Ill. (H. A. Van Wassen, Duquesne Light Co., Pittsburgh 19, Pa.)

8–10. Mathematical Theories of Biological Phenomena, symp., New York, N.Y. (N. Rashevsky, Committee on Mathematical Biology, 5741 Drexel Ave., Chicago 37, Ill.)

8–12. American College of Physicians, 42nd annual, Miami Beach, Fla. (American College of Physicians, 4200 Pine St., Philadelphia 4, Pa.)

8–12. American Psychiatric Assoc., 117th annual, Chicago, Ill. (C. H. H. Branch, 156 Westminster Ave., Salt Lake City, Utah)

9–11. Western Joint Computer Conf., Los Angeles, Calif. (W. F. Bauer, 843 Fallbrook Ave., Canoga Park, Calif.)

10–12. Production Engineering Conf., Toronto, Canada. (R. B. Larson, 5701 Carnegie Ave., Cleveland 3, Ohio)

10–13. National Science Fair—International, 12th, Kansas City, Mo. (Science Service, 1719 N Street, NW, Washington 6, D.C.)

11–13. Acoustical Soc. of America, Philadelphia, Pa. (W. Waterfall, 335 E. 45 St., New York 17)

11–13. American Inst. of Industrial Engineers, annual, Detroit, Mich. (W. J. Jaffe, Newark College of Engineering, 367 High St., Newark 2, N.J.)

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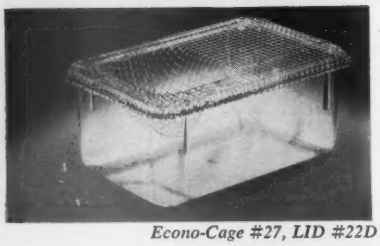

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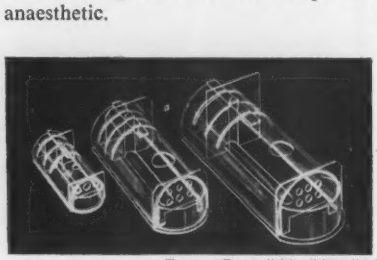
Two new pieces of animal restraining equipment are now available from the Econo-Cage Division of Maryland Plastics, Inc. A small restrainer for mice weighing from 10 to 40 grams (Econo-Cage #88), and a large unit for rats and hamsters weighing from 250 to 600 grams (Econo-Cage #91), are new additions which supplement Econo-Cage #90 for 150 to 300 gram rats and hamsters.

These clear acrylic plastic units afford rapid and safe immobilization of animals, easy access and maximum visibility of animals in restraint. Econo-restrainers prevent unanesthetized animals from attacking tubes, cannulae, and other fixtures; provide extended housing during nutritional studies; restrain animals during administration of intravenous, intraperitoneal, intramuscular, and subcutaneous injections; and are useful for administering intravenous fluid drips and anaesthetic.



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All three sizes have an adjustable tailgate which fits into any of three slots to vary cage length, confine the animal, and serve as a cage door. Openings at the top, bottom, and tail provide easy access to any part of the animal (the bottom slot also permits drainage of animal waste). A hopper permanently attached to the front of the unit includes a trough for granular feeds and a water tube inlet.

The small restrainer, Econo-Cage #88, can be varied from 2″ to 3½″ in length and is 1¼″ wide. The medium restrainer, Econo-Cage #90, can be varied from 4½″ to 6″ in length and is 2½″ wide. The large restrainer, Econo-Cage #91, can be varied from 5″ to 7″ in length and is 3″ wide. All these units can be cleaned chemically or with hot water, they are not autoclavable.

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11–13. American Radium Soc., Colorado Springs, Colo. (C. G. Stetson, 350 Engle St., Englewood, N.J.)

13–14. Society for Economic Botany, annual, Cambridge, Mass. (R. E. Schultes, Botanical Museum of Harvard University, Cambridge 38)

15–16. Co-ordination Compounds, symp., Hamilton, Ontario, Canada. (R. J. Gillespie, McMaster Univ., Hamilton)

15–17. Institute of Radio Engineers, natl. symp., Washington, D.C. (G. Shapiro, National Bureau of Standards, Washington 25)

15–17. Radiation Research Soc., annual, Washington, D.C. (E. L. Powers, Div. of Biological and Medical Research, Argonne National Laboratory, Argonne, Ill.)

15–18. Society of Aeronautical Weight Engineers, Akron, Ohio. (D. B. Block, 4004 Oxford Ave., NW, Massillon, Ohio)

15–18. Spectroscopy, 12th annual symp., Chicago, Ill. (W. Ashby, Continental Can Co., Inc., 7622 S. Racine Ave., Chicago 20, Ill.)

15–20. Conference on Nuclear Electronics, Belgrade, Yugoslavia. (J. Burt, International Atomic Energy Agency, United Nations, New York, N.Y.)

16–18. Western Conf. on Anesthesiology, biennial, Portland, Ore. (J. O. Branford, 2307 NW Overton St., Portland 9, Ore.)

17–20. American College of Cardiology, New York, N.Y. (P. Reichert, 350 Fifth Ave., Empire State Bldg., New York 1)

18–20. Host Tumor Interactions, intern. symp., Detroit, Mich. (M. J. Brennan, Oncology Div., Henry Ford Hospital, Detroit 2)

22–24. American Thoracic Soc., Cincinnati, Ohio. (F. W. Webster, 1790 Broadway, New York 19)

22–24. Global Communications, 5th natl. symp., Chicago, Ill. (R. D. Slayton, 5555 Touhy Ave., Skokie, Ill.)

22–24. Telemetering Conf., natl., Chicago, Ill. (J. Becker, AC Spark Plug Division, General Motors Corp., Milwaukee 1, Wis.)

22–25. American Urological Assoc., Los Angeles, Calif. (W. P. Didusch, 1120 N. Charles St., Baltimore 1, Md.)

22–25. Design Engineering Conf. and Show, Detroit, Mich. (ASME Meetings Dept., 29 W. 39 St., New York 18)

22–25. National Tuberculosis Assoc., Cincinnati, Ohio. (J. G. Stone, 1790 Broadway, New York 19)

22–26. Engineering Conf. and Exhibit, 29th annual, New York, N.Y. (G. E. Seeley, ASTME Headquarters, 10700 Puritan Ave., Detroit 38, Mich.)

22–26. Society of Photographic Scientists and Engineers, annual, Binghamton, N.Y. (M. G. Anderson, Ansco, Vestal Parkway East, Binghamton, N.Y.)

22–27. International Acad. of Legal Medicine and of Social Medicine, 5th cong., Vienna, Austria. (M. Helpern, Chief Medical Examiner, City of New York, 55 East End Ave., New York 28)

23–25. Large Capacity Memory Techniques for Computing Systems, symp., Washington, D.C. (Miss J. Leno, Code 430A, Office of Naval Research, Washington 25)

25. Gastroenterology Research Group, Chicago, Ill. (N. C. Hightower, Scott and White Clinic, Temple, Tex.)

25–26. Medical Technology, symp., Cleveland, Ohio. (J. W. King, Cleveland Clinic, 2020 E. 93 St., Cleveland 6)

25–26. Nitro Aliphatic Chemistry, symp., Lafayette, Ind. (Purdue Memorial Union, Purdue Univ., Lafayette)

25–26. Operations Research Soc. of America, 9th annual, Chicago, Ill. (D. Schiller, Gaywood-Schiller Associates, 203 N. Wabash Ave., Chicago 1)

26–27. American Otological Soc., Lake Placid Club, Essex County, N.Y. (J. A. Moore, 525 E. 68 St., New York 21)

26–27. Biology of the Trachoma Agent, symp., New York, N.Y. (F. B. Gordon, Natl. Naval Medical Center, Bethesda 14, Md.)

26–27. Cardiovascular Tissue Culture Conf., Dover, Del. (O. J. Pollak, Dover Medical Research Center, Inc., P.O. Box 228, Dover)

26–3. American Acad. of Dental Medicine, cruise to Bermuda and Nassau. (H. Ward, 15 Bond St., Great Neck, N.Y.)

28–1. Special Libraries Assoc., 52nd annual, San Francisco, Calif. (B. M. Woods, SLA, 31 E. 10 St., New York 3)

29–31. American Gynecological Soc., Colorado Springs, Colo. (A. H. Aldridge, 899 Park Ave., New York 21)

29–31. Cancer Symp., 6th annual, Regina, Saskatchewan, Canada. (A. J. S. Bryant, Allan Blair Memorial Clinic, Regina)

29–31. Chemical Inst. of Canada, 44th annual, Ottawa. (Chemical Inst. of Canada, 48 Rideau St., Ottawa 2)

29–3. Molecular Spectroscopy, 5th European cong., Amsterdam, Netherlands. (D. H. Zijp, Secy., Nieuwe Achtergracht 123, Amsterdam-C.)

30. Nutrition Soc. of Canada, 4th annual, Guelph, Ontario. (E. V. Evans, Dept. of Nutrition, Ontario Agricultural College, Guelph)

31–2. Canadian Federation of Biological Societies, Guelph, Ontario, Canada. (E. H. Bensley, Montreal General Hospital, 1650 Cedar Ave., Montreal 25, P.Q.)

31–2. Radar symp., 7th annual, Ann Arbor, Mich. (Coordinator, 7th Annual Radar Symposium, Institute of Science and Technology, Box 618, Ann Arbor)

June

2–3. Canadian Soc. for Clinical Chemistry, annual general meeting, Guelph, Ont. (C. R. Cameron, Ontario Veterinary College, Guelph)

2–5. Latin-American Congress of Physical Medicine, Lisbon, Portugal. (C. Lopez de Victoria, 245 E. 17 St., New York, N.Y.)

3–11. Medical-Surgical Film Festival, 4th intern., Turin, Italy. (Minerva Medica, Corso Bramante 83–85, Turin)

4–9. Mass Spectrometry, ASTM Committee E-14, Chicago, Ill. (G. Crable, Gulf Research Center, P.O. Box 2038, Pittsburgh 30, Pa.)

4–10. World Congress of Psychiatry, 3rd, Montreal, Canada. (A. Roberts, Allan Memorial Inst., 1025 Pine Ave. West, Montreal 2, P.Q.)

4–12. Cellular Regulatory Mechanisms, 26th Cold Spring Harbor Symp. on Quantitative Biology, Cold Spring Harbor,

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5–7. American Soc. for Quality Control, Philadelphia, Pa. (W. P. Youngclaus, Jr., 161 W. Wisconsin Ave., Milwaukee 3, Wis.)

5–7. Fundamental Aspects of Radiosensitivity, symp., Upton, N.Y. (A. H. Sparrow, Brookhaven National Laboratory, Upton, L.I.)

5–8. International Instrument-Automation Conf. and Exhibit, Instrument Soc. of America, Toronto, Canada. (I.S.A., 313 Sixth Ave., Pittsburgh 22, Pa.)

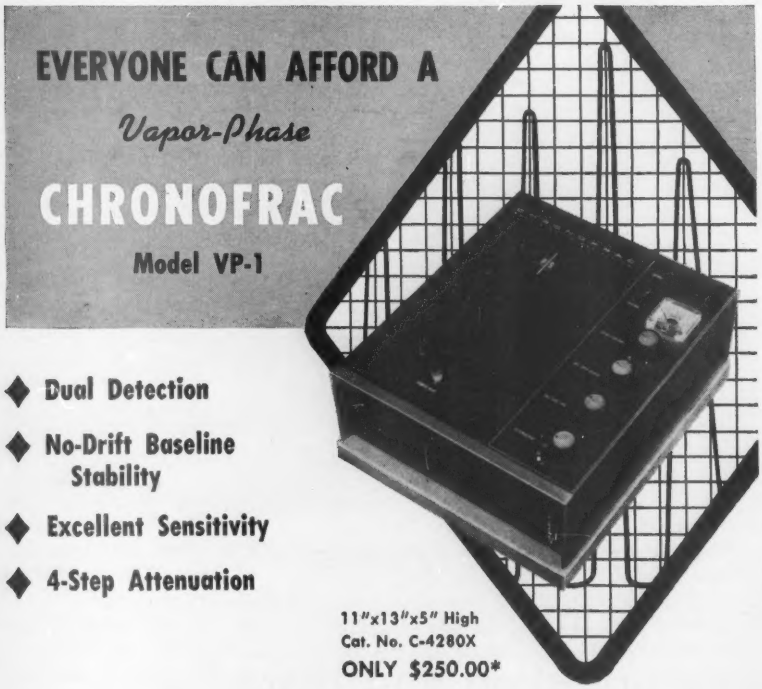
5–9. Animal Reproduction, 4th intern. cong., Amsterdam, Netherlands. (Secretariat, Burgemeester de Monchyplein 14, The Hague, Netherlands)

5–9. Effects of Ionizing Radiation on the Nervous System, symp., Vienna, Austria. (J. Burt, International Atomic Energy Agency, United Nations, New York, N.Y.)

5–10. International Colloquium on Spectroscopy, 9th, Lyon, France. (Secretariat, Groupement pour l'Avancement des Méthodes Spectrographiques, 1, rué Gaston Boissier, Paris 15)

5–16. Operations Research and Systems Engineering, Baltimore, Md. (Dean, School of Engineering, Johns Hopkins Univ., Baltimore 18)

6–8. Tissue Culture Assoc., 12th annual, Detroit, Mich. (F. E. Payne, Dept. of Epidemiology, Univ. of Michigan, Ann Arbor)



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8–11. American Electroencephalographic Soc., Atlantic City, N.J. (G. A. Ulett, Malcolm Bliss Mental Health Center, 1420 Grattan, St. Louis 4, Mo.)

8–18. International Organization for Standardization, general assembly (members only), Finland. (American Standards Association, 70 East 45 St., New York 17)

9–11. Society of Biological Psychiatry, Atlantic City, N.J. (G. N. Thompson, 2010 Wilshire Blvd., Los Angeles 57, Calif.)

9–17. European Convention of Chemical Engineering, Frankfurt, Germany. (DECHEMA, Postfach No. 7746, Frankfurt/Main 7)

11–15. American Soc. of Mechanical Engineers, summer annual, Los Angeles, Calif. (O. B. Schier II, 29 W. 39 St., New York 18)

12–13. Radio Frequency Interference, 3rd natl. symp., Washington, D.C. (E. F. Mischler, National Engineering Service, Washington, D.C.)

12–14. American Dairy Science Assoc., Madison, Wis. (H. F. Judkins, 32 Ridgeway Circle, White Plains, N.Y.)

12–14. American Neurological Assoc., Atlantic City, N.J. (M. D. Yahr, Neurological Inst., 710 W. 168 St., New York 32)

12–14. Society for the Study of Development and Growth, regeneration symp., Williamstown, Mass. (A. C. Braun, Rockefeller Inst., New York 21)

12–15. Nature of the Real, conf., Milwaukee, Wis. (E. D. Simmons, Dept. of Philosophy, Marquette Univ., Milwaukee 3)

12–15. Physics of Electronic and Atomic Collisions, intern. conf., Boulder, Colo. (B. Bederson, Physics Dept., New York Univ., New York 53)

12–16. Association of Official Seed Analysts, Richmond, Va. (D. D. Forsyth, Agronomy Building, Madison 6, Wis.)

12–16. Molecular Structure and Spectroscopy, symp., Columbus, Ohio. (R. A. Oetjen, Dept. of Physics and Astronomy, Ohio State Univ., Columbus 10)

12–18. European Assoc. for Animal Production, 8th intern. cong., Hamburg, Germany. (European Assoc. for Animal Production, Via Barnaba Oriana 28, Rome, Italy)

12–24. European Inst. of Scientific Studies for the Prevention and Treatment of Alcoholism, Amsterdam, Netherlands. (D. Ehlbeck, Intern. Bureau against Alcoholism, Case Gare 49, Lausanne, Switzerland)

12–29. Statistical Quality Control Intensive Courses for the Chemical and Processing Industries, 18th annual, Rochester, N.Y. (H. M. Kentner, Extended Services Div., Rochester Inst. of Technology, Rochester 8)

13–14. Product Engineering and Production, 5th natl. conf., Philadelphia, Pa. (P. J. Riley, R.C.A., Building 10-6, Camden 2, N.J.)

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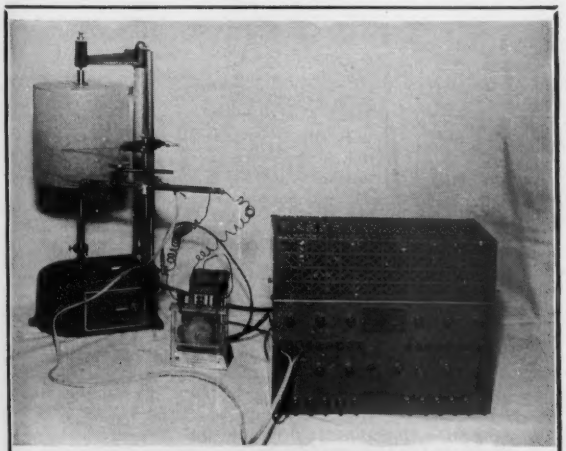

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14–17. American Assoc. of Bioanalysts, Dallas, Tex. (L. D. Hertert, 490 Post St., Room 1049, San Francisco 2, Calif.)

16–17. Meteoritical Soc., Nantucket, Mass. (G. L. Rowland, Long Beach City College, Long Beach 8, Calif.)

17–21. American Nuclear Soc., Boston Mass. (O. J. Du Temple, ANS, 86 E. Randolph St., Chicago 1, Ill.)

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18–23. American Meteorological Soc., 193rd natl., and Pacific Div., AAAS, 42nd annual, Davis, Calif. (AMS, 45 Beacon St., Boston 8, Mass.)

18–23. American Soc. of Medical Technologists, Seattle, Wash. (Miss R. Matthaei, Suite 25, Hermann Professional Bldg., Houston 25, Tex.)

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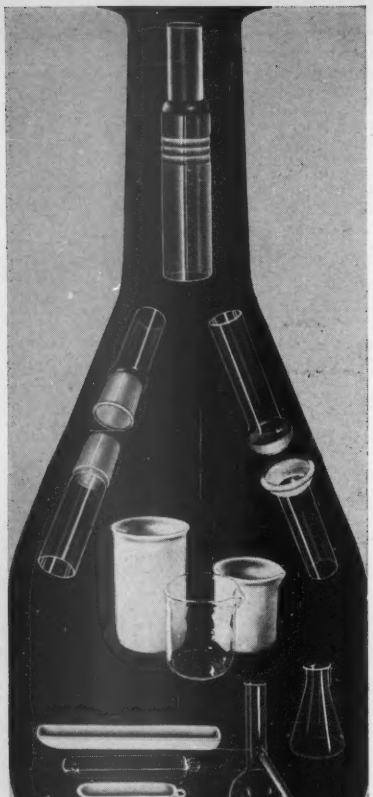

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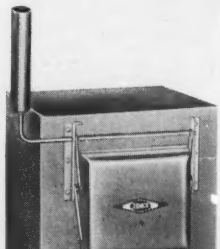

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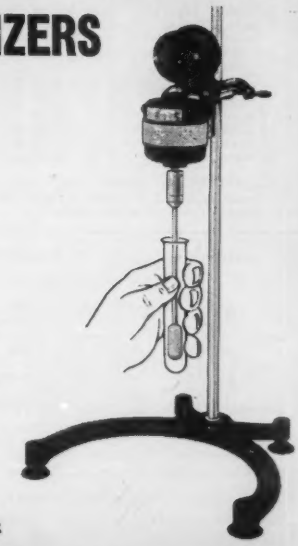

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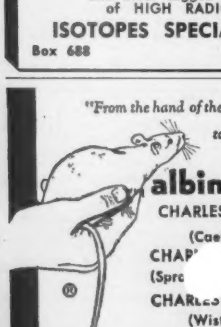

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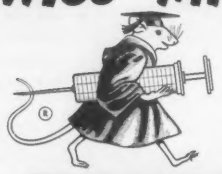

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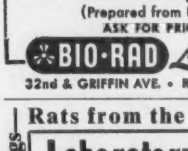

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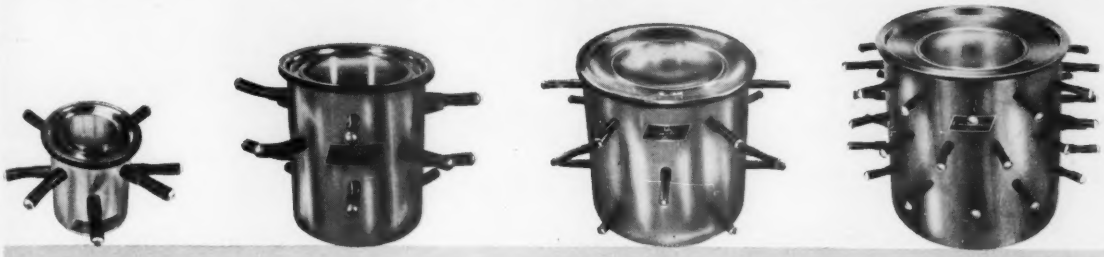

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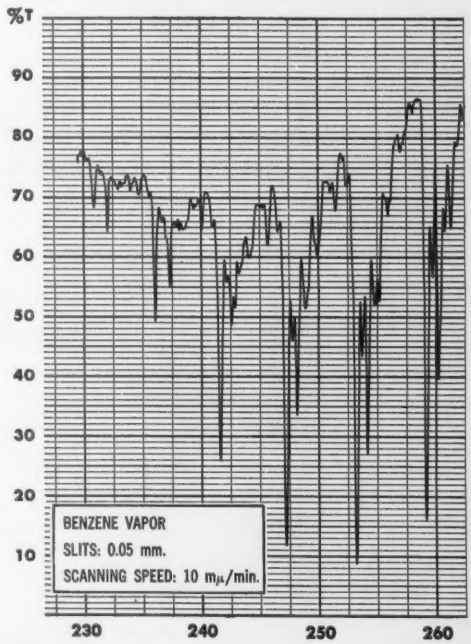

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